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Foundations Of Operant Behaviour Theory

Mitchell Drew Mccrimmon

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FOUNDATIONS OF OPERANT

BEHAVIOUR THEORY

by

Mitchell Drew McCrimmon

Department of Philosophy

Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

Faculty of Graduate Studies
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Mitchell Drew McCrimmon

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ABSTRACT

The problem to which this essay is addressed consists in our lack of an adequate picture of the status of B. F. Skinner's operant behaviour theory. On the one hand, the theory is claimed to have substantial implications and applications, while on the other, critics allege it to be devoid of relevance to human action.

Unfortunately, inadequate analyses of scientific change or reduction make it nearly impossible to assess the theory. The strategy adopted in the present study involves the utilization of what is currently viewed as a relatively acceptable theory of scientific change in order to sort out those contentions of Skinner's and his critics which are unsupportable from those with some substance.

Included in the investigation are detailed comparisons of the central concepts and distinctions of action theory with counterparts in operant theory. Chomsky's criticisms, foundational issues in learning theory, and putative implications for the foundations of social science, are also discussed.

The result is that, while many of Skinner's contentions cannot be accepted if interpreted literally, there is nevertheless substantive core to the theory which is of very significant interest to those concerned with the understanding of human action.

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I. INTRODUCTION

Philosophical interest in behaviourisms of various sorts has declined in recent years. Not too long ago some version of behaviourism was regarded by many as the only sound approach in psychology; no other methodological standpoint could begin to satisfy empiricist scruples. Others saw in behaviourism the key to metaphysical questions pertaining to the philosophy of mind and language. Since virtually all such behaviouristic positions are by now fairly generally viewed as misguided, a serious reconsideration of a version of behaviourism cannot but seem heroic, or as an endeavour to beat a dead horse. Yet I am proposing to examine just such an approach to psychological inquiry and a specific substantive theory of human action, that is, ostensibly at least, one of the most radical of behaviourisms ever developed, namely, that of B. F. Skinner.

On the whole, philosophers¹ have found Skinner's claims very interesting but quite plainly false. Interest has been generated by Skinner's contention that his "operant behaviour theory" (operant theory for short) is a successful account of human action that is thoroughly deterministic, mechanistic, materialistic, and in line with the narrowest of empiricist presuppositions. The reasons philosophers have rejected Skinner's radical behaviourism are many. Most

fundamental is surely the felt inadequacy of the overly narrow empiricism that appears to serve as the foundation of Skinner's operant theory. No attempt to reduce, define, or in any way translate, psychological terms or statements into observation language has been remotely successful. Moreover, and this is a second source of skepticism, critics such as Charles Taylor have shown that our everyday understanding of human action is inescapably teleological. No mechanistic treatment of human action could conceivably replace such a framework, or so it would seem.

What I propose is to extract what strikes me as the valuable core of operant theory and to reconstruct its methodological and philosophical foundations so as to dispose of the above two charges. I shall argue that there is an interpretation of operant theory that is incompatible with the narrow empiricism from which it originated historically and which combines mechanistic and teleological features in such a manner that the usual sharp contrast between these two frameworks loses its force entirely. Since the resulting theory is without its distinctively radical behaviouristic flavour, I shall endeavour to regenerate its philosophical interest. In this vein I hope to show that there is an affinity between operant theory and the theory of evolution by natural selection². The result is that the former can be disassociated from empiricist epistemology and employed in the development of a type of epistemology

that is very much in the air today, namely, an evolutionary epistemology.

An essential ingredient in my strategy will be what I consider to be an adequate theory of reduction and scientific change. There are basically two reasons for appealing to such a theory. First, unless I can show that the foundational and substantive revisions to operant theory I want to propose are natural or to be expected, then they are bound to appear somewhat ad hoc. Second, all critics and defenders of Skinner and other behaviourisms presuppose, either explicitly or implicitly, some analysis of reduction or scientific change. There are two extreme theories of the nature of scientific change. Oddly enough, both opponents and defenders of behaviouristic approaches to psychology have adopted, or could adopt, either extreme in defending their position.

The conservative extreme maintains that anything which counts as part of scientific progress does so by virtue of being an increment in an entirely cumulative process. The transition from one theory to another is such that the successor theory preserves, in some sense or other, the entire content of its immediate predecessor. Anything not falling within the scope of this incremental process simply is not science. False theories are not replaced; we merely dispense with prescientific beliefs as real science emerges. Now an opponent of behaviourism, and Skinner in

particular, can cite this analysis of scientific change; he can point to allegedly irreplaceable facts about human consciousness and self-direction; he can argue persuasively that operant theory fails to capture the full sense of rational intentional action, and thereby conclude that operant theory is totally inadequate. With precisely the same theory of scientific change in hand, a defender of the behaviourist programme would simply deny the scientific character of the introspectively revealed "facts" of consciousness. All talk of the mental, self-direction, intentionality, teleology, etc., is part of a mistaken pre-scientific tradition. Science is cumulative, but it has no interest in accumulating pre-scientific, not to mention anti-scientific, relics. There is no difficulty understanding how each side of the dispute feels inclined to see the other as doomed to never ending question-begging. The relation between the latter view of the matter and empiricism is also apparent.

The analysis of scientific change at the opposite extreme is radical with respect to the nature of inter-theory relations. This view is typically associated with Feyerabend and holds that the relation between successive theories in the history of science is one of incommensurability; science is in no sense cumulative. As there is no clear cut distinction between science and pre-science on this view, defenders of Skinner employing it need not

regard non-behaviouristic theories of human action as mere superstition. Our commonsense conception of human action is, or at least embodies, a respectable theory, but since scientific change results between incommensurables, the impotency of operant theory to do any justice to our intuitions is just so much the worse for our intuitions. This view has the virtue that it does not rest upon an inadequate empiricist analysis of the history of science, or, for that matter, upon any empiricist doctrines. But this gain is costly: we are left without any grounds for preferring behaviourism or any other theory over the conceptual framework underlying our commonsense understanding of human action.

Accordingly, opponents of Skinner can also adopt a Feyerabendian analysis of scientific change and reply to defenders of Skinner in either one of two ways. They can point to Charles Taylor's demonstration of incompatibility between mechanism and teleology and rightly argue that they must surely be offered some reason for giving up a deeply entrenched and highly successful theory. Or they can adopt a more meta-philosophical stance and argue that because of the very nature of inter-theory relations, no reason could ever be offered for abandoning the basic categories in terms of which we understand human action. Finer grained analysis is possible, but nothing resembling radical displacement.

Clearly, disputants arguing their case from either theory of scientific change are deadlocked. Besides the inadequacy of both theories to present a coherent characterization of the history of science, the rationale for employing a different theory of scientific change is apparent. With either of the extreme analyses of theory change in hand, opponents of Skinner completely fail to see so much as the possibility of anything resembling theory change. Defenders of Skinner and other behaviourisms are forced to adopt severe measures simply in order to get a research programme off the ground. The possibility of a synthesis of competing conceptual frameworks never arises because advocates of both theories of scientific change are completely polarized in their view of existing alternatives. On one extreme there is a sharp contrast drawn between science and non-science. And at the other end of the continuum there is a sharp contrast between specific theories within science. So long as some such polarization is our only option, it is difficult to see how we could ever get beyond the level of question-begging polemic.

Since, it seems to me, operant theory is just such a beginning at a synthesis of conceptual frameworks that are usually regarded as diametrically opposed, it is extremely fortunate that more suitable analyses of scientific change are finally emerging. The way is clear for reconstructing the foundations of operant theory in the light of this.

conception of science without having to adopt either a narrow empiricism or a view which portrays science as an irrational enterprise. It will develop that an adequate theory of scientific change not only points in the direction of a synthesis of behaviouristic and non-behaviouristic frameworks, but that such a theory is itself a synthesis of opposing analyses of scientific change. The result will be that operant theory can replace current theories of human action, but only by making a substantial body of concessions, so much so that the term "replacement" is a bit of a misnomer. However, the rationale for deploying an adequate analysis of scientific change in this context is to legitimate proposed concessions as natural rather than as ad hoc. If such a synthesis proved fruitful both sides of the current dispute between behaviourism and its critics would view it as a victory for their side. But this would be to miss much of the point of the present argument.

The analysis of scientific change and the theory of human action to be developed in what follows are meant to form a coherent whole along with an appropriate evolutionary epistemology to be briefly sketched later. It is suggestive to notice that the current dispute over behaviourism tends to be a clash between static frameworks. A similar picture of science is hard to shake off. One thing the analysis of science here adopted has in common with the interpretation of operant theory to be proposed is an emphasis on understanding

developmental change.

Accordingly, I shall argue that operant theory has two fundamental aspects. Its immeasurably more significant aspect pertains to the generation and change of act-types, the less important side relating to the explanation of act-token occurrences. This latter role will be shown to be best played by an internal state causal theory of action such as that of Alvin Goldman³ with little more than operant qualifications introduced. Nonetheless I shall argue that operant theory is more fundamental than the qualified version of Goldman's theory, precisely because of its developmental aspect. This is in keeping with the larger philosophical outlook I am suggesting. Because of the generally evolutionary epistemological standpoint I am adopting it is natural to downplay the explanation of particular events in terms of static structure in favour of an understanding of the process of change.

For a number of reasons that should by now be rather obvious, I do not propose to attempt anything resembling a final or definitive assessment of operant theory. First, I am convinced that such evaluation requires formal treatment; but attempts to formalize operant theory are only beginning. The way in which operant theory might be developed theoretically and conceptually so as to mesh with other theories of human action is barely in the exploratory stage; this is my second reason. Finally, I reject the implicit

a priori approach to philosophy of science that underlies such a notion of assessment or evaluation. In other words the function of philosophy of science as envisaged herein is not only to criticize completed theories but also to contribute critically to their ongoing development. My aim is thus neither a priori analysis of methodological constraints nor final assessment. There simply is no sharp line between evaluation and proposal for change. The project I am proposing to undertake is therefore a mixture of the two, and one in its very early stages at that. The main result I hope to achieve is to generate greater philosophical interest in operant theory.

The first step of my argument consists in a sketch of what I consider to be the most defensible theory of scientific change available. I shall largely content myself with assuming that this theory is independently reasonable by virtue of its ability to adequately portray the history of science. The main ingredient of this section will be a comparison of the general sorts of implications alternative analyses of theory change have for the structure and foundations of operant theory.

The second stage of my argument presupposes the above mentioned theory of scientific change. The central claim of this theory is that successive theories in the history of science can begin their developmental histories as quite radical incommensurables, but that in the process of one

theory coming to succeed the other, both theories will converge upon a single reconciliation point. Both successor and predecessor are thus highly qualified versions of their originals. Before suggesting in detail the way in which operant theory might be revised so as to succeed and in turn revise action theory, it is appropriate to contrast the two extreme versions taken by their originals. The central point of this section is to show how both theories have already been converging. I shall largely focus on early behaviourism in order to demonstrate the fact that operant theory has left its cruder origins behind.

The third stage of my argument involves coming to grips with a detailed comparison of the structure of operant theory with the structure of Alvin Goldman's theory of human action. With the analysis of theoretical change constantly in mind, I shall attempt to establish that operant theory already bears a great deal of structural resemblance to action theory. To this end I shall compare the central concepts of the two theoretical frameworks: act and operant, rule and contingency of reinforcement. I shall again employ the analysis of theory change to justify the inclusion in operant theory of a causal role for internal events and states of organisms in the explanation of behaviour. The last part of this section will be a brief sketch of the evolutionary character of operant theory including a few speculative comments on the implica-

tions of this result.

Putative limitations of operant theory that are of substantial foundational relevance will be explored in the fourth section. Learning by a process known as "modeling", a form of learning by observation, will be discussed as a possible anomaly to a strict operant conditioning account of all behaviour modification. I shall also comment on the question of self-direction and the allegedly essential role of awareness in learning. The implications of the emerging interpretation of operant theory for certain of its standard criticisms will be sketched.

One way of generating interest in a scientific theory is to argue for its generality. Thus if it can be shown not only that operant theory is a reasonable successor to action theory, but also that it is a plausible candidate upon which to found the rest of the social sciences, then the theory will immediately demand more serious attention from philosophers than it has as yet received. Accordingly, my fifth section is designed as an exploration of just this possibility. The main ingredients are critical remarks on the work of the sociologists George Homans and Richard Emerson, both of whom have taken significant steps toward the realization of founding a general theory of social behaviour upon operant theory. Included in this section will be a consideration of a theory of rational choice and its possible relation to operant theory. Among the

questions to be discussed are: Is the notion of maximization of expected utility more fundamental than any comparable notion in operant theory? If so is not some theory of rational choice the real successor to current theories of action as well as the most suitable candidate for a foundation of the social sciences? If not, then is operant theory the foundation for a theory of rational choice? One of the reasons these questions are difficult to answer is that Homans and Emerson fail to see fully the developmental aspect of operant theory. They are good methodological individualists. Moreover, theories of rational choice do not pertain primarily to the generation and change of act-types. Nonetheless, it will emerge that, in differing senses, it is possible to answer each of the above questions affirmatively.

In my concluding comments plausible consequences and implications will be drawn; speculative suggestions for further research will also be made.

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¹ Hamlyn, D. W., "Conditioning and Behaviour", in Borger and Cioffi (eds.), Explanation in the Behavioural Sciences, Cambridge University Press, 1970 (for example).

² Skinner, B. F., "The Phylogeny and Ontogeny of Behaviour", in Skinner, Contingencies of Reinforcement: A Theoretical Analysis, Appleton-Century-Crofts, 1969.

³ Since nothing significant hangs on my comparing operant theory with Goldman's analysis of action as opposed to someone else's, I have adopted many of Goldman's terms and distinctions without definition or evaluation. However, a few qualifications are in order. Although I adopt Goldman's definition of act-types, where he relies on the concept of a want I would refer to whatever structural state takes the place of wants in the envisaged future operant theory. Secondly, for comparative purposes I can restrict myself to basic acts which are bodily movements. The status of mental acts will have to be left open.

Lastly, although I distinguish between acquired and unacquired acts and associate the latter with basic acts, nothing hangs on there being some acquired basic acts. I am only concerned to discuss comparatively a significant portion of typical bodily actions rather than every conceivable case.

II. REDUCTION AND SCIENTIFIC CHANGE

Before I begin presenting a detailed analysis of reduction and scientific change, it is of the utmost importance to state how the sort of reduction I am going to suggest differs from what is usually regarded as a behaviouristic reduction. Without such a qualification, my claim to be extracting a valuable core of operant theory out of its radical empiricist setting will appear mysterious at best. The notion of a behaviouristic reduction as it is viewed by philosophers, and Skinner as well, is one involving the rejection, by some process of reduction or replacement, of all theoretical language in favour of a pure observation language. To the extent that psychological language pertaining to mental states is worth preserving at all, it must be translatable into, and hence reduced to, observation language. Much of Skinner's polemic has been precisely to this end: A proper science of behaviour must restrict itself to observable events and the search for functional relations between them. Now it is precisely this positivistic sort of reduction that I am rejecting entirely.

Nevertheless, an adequate theory of scientific change will show that there is something to Skinner's rejection of the particular theory of mental states (to the extent that such can be singled out) that underlies our commonsense

conception of the explanation of human action. But once the usual type of empiricist motivated behaviouristic reduction is rejected, the door is open to a more standard kind of reduction akin to those abundant in the history of physical science. Accordingly, it is not entirely inappropriate to think of operant theory, as many philosophers do, on the analogy with thermodynamics. Both are "molar" theories; both are reducible to a structural theory. But this analogy breaks down in a fundamental manner; for it overlooks the fact that operant theory has two basic aspects. One aspect pertains to the explanation of particular acts at particular times and is clearly like thermodynamics in this respect. In both cases molar behaviour can be explained by reference to a fixed structure. On the other hand, operant theory also purports to explain the generation and change of molar act-types and thereby the alteration of underlying structure as well.

One reason for the deployment of a theory of reduction in this context is simply in order to show how that aspect of operant theory that pertains to the explanation of the occurrence of particular acts must be supplemented by some theory of the role of internal states in order to replace a theory such as Goldman's. An adequate theory of scientific change is necessary in order to ward off charges of trivial verbal transition and ad hocness. A second reason is that some sense must be made of how the above type of theory can

be subsumed under a developmental operant theory. Further characterization of the differences between these two aspects of operant theory would be premature at this point. It is sufficient for present purposes to emphasize that the sort of reduction or theory change to be aimed at in what follows is in no interesting sense behaviouristic. Rather, it involves the replaceability of an internal state causal theory of human action such as Goldman's by a very similar operant theory supplemented by a theory of the relation between internal structure and behaviour. The result will be that the conceptual shift involved is not tantamount to wither an acceptance of empiricist scruples or mechanism.

The task of the present stage of my argument, to which I can now turn, consists in presenting a reasonable analysis of scientific change showing how its adoption supports a viewpoint with respect to operant theory that differs from those accompanied by other analyses of theory change. I have already indicated the need for such a theory, but strictly speaking a very radical sort of theoretical change such as Feyerabend's analysis might licence (and such as critics like Charles Taylor find inconceivable) cannot be ruled out a priori. My aim is certainly not to dictate or predict the future course of science. Instead, I intend to show that there is an analysis of scientific change and an interpretation of operant theory

such that there is a sense in which operant theory could succeed current theories of action without our having to accept either Feyerabend's radical thesis or the narrow empiricist thesis he rightly criticizes. I shall now endeavour to answer the following question: How, specifically, does the reasonable theory of scientific change alluded to above differentially suggest what particular sorts of theoretical changes?

In my answer to this question I shall rely heavily on two recent proposals pertaining to the nature of scientific change that differ mostly in point of emphasis. I have in mind the seemingly quite independent efforts of Noretta Koertge¹ and Kenneth Schaffner². Although both authors arrive at an analysis of inter-theory relations that is the same or nearly so, Schaffner is primarily interested in the nature of the relation between completed theories.

Koertge's theory pertains more to the heuristics of the developmental process by which a theory can change so as to succeed another. These two perspectives complement each other perfectly for my purposes. I want to say something about both of these aspects of theory change; I want to make some claims about the way in which a completed operant theory would relate to other completed theories about the behaviour of organisms. And I also want to show that operant theory has been developing, and could continue to develop, in a way that is entirely non-arbitrary and non-

trivial according to the general theory of such a developmental process.

I shall first discuss the way in which the Koertge-Schaffner view of the relation between completed theories differs from others. One interesting feature of Schaffner's treatment of reduction is that he produces a general theory of reduction that yields every major competing analysis as a special case. The merit of this result is that which special case operant theory turns out to exemplify most closely can be left as the empirical matter it surely is. Two of his special cases are just the two extremes mentioned above that are usually attributed to Nagel and Feyerabend respectively. The important result is not so much that these extreme views are never exemplified in the history of science but rather that scientific change is generally a much more subtle and complex process than either of the above two cases indicates.

The reason an a priori adoption of Nagel's type of reduction paradigm leads to a polarized deadlock has already been mentioned. Schaffner characterizes this Nagel-Woodger-Quine analysis of reduction as follows:

Essentially this account of reduction can be characterized as direct reduction -- in which the basic terms (and entities) of one theory are related to the basic terms (and entities) of the other, (assuming that the reduced theory is an adequate one) and the axioms and the laws of the reduced theory are derivable from the reducing theory.³

Thus thermodynamics can be reduced to statistical mechanics because thermodynamics is an independently adequate theory. Now if this view of reduction is combined with the usual sharp contrast between science and non-science, the usual behaviourist move is simply to deny that teleological-mentalistic theories of human action are adequate theories. They thus deny that an adequate behavioural theory has to be one that fits into the conceptual framework of the teleological action theorist. An adequate behavioural theory can be developed with the intent of reducing it to future control state neurophysiological theory.

The opponent of all behaviourisms supposes he can dictate what sort of behavioural theory will be adequate by presupposing the adequacy of his reducing theory (he may not think of it as a reducing theory but he will surely think of it as explanatory of behaviour in some sense). Employing Nagel's analysis of reduction and presupposing that some commonsense-respecting theory of action is adequate, the opponent of neurobehaviouristic theories can argue that his theory cannot be superseded by any theory which, in Schaffner's terms, is not a direct reduction. No theoretical change will be forthcoming which does not preserve the full sense of this allegedly adequate theory. An example of this sort of reactionary stance that can, at least in principle, go hand in hand with such an analysis of reduction is D. W. Hamlyn's recent attempt to rule out

the replacement of commonsense learning theory by any behaviourist conditioning theory. Referring to conditioning studies he argues that they:

...involve...the animal or person taking something; e.g. as a sign of something else; they involve the idea that the animal or person must derive information from its environment and put it to use. This cannot be expressed in stimulus-response terms. But only a story of this kind will be relevant to behaviour.⁴

The reason Hamlyn and the behaviourist are deadlocked is simply that the latter can adopt the same analysis of scientific change and argue that the sort of "story" Hamlyn claims must be told is akin to pre-scientific stories and can be safely ignored as such.

The deadlock is not weakened by the adoption by the disputants of a Feyerabendian analysis of scientific change. The behaviourist continues to ignore his opponent's intuitions but now on different grounds; now he does not have to attribute pre-scientific beliefs to his opponent; he just has to maintain the falsity of his opponent's theory and its incommensurability with his own.

The basic insight of both Koertge and Schaffner is to see that the adoption of the incommensurability notion is simply going too far. Science is cumulative, much as Nagel and others believed. And Feyerabend's view was not all that consistent in any case. He argued for radical incommensurability while still maintaining that theories could not only

meaningfully be said to succeed other theories, but even that successor theories corrected their predecessors and explained why they worked. Koertge and Schaffner take this latter idea as a significant advance over Nagel, but by abandoning incommensurability, develop it into a theory of reduction. There emerges a sense in which science is cumulative or progressive but in which science can encompass theoretical change as well. Successor theories do not either directly reduce their predecessors or completely replace them. Rather they directly reduce a qualified version of their predecessors. Successive theories thus have a relation between them that Schaffner calls "strong analogy" and that Koertge calls, after H. R. Post, a "general correspondence". The upshot is that, radical behaviourists to the contrary, the entire content of competing theories does not have to be rejected, and opponents of behaviourism to the contrary, theoretical change that does not capture the full sense of our intuitive conception of teleology, intentionality, self-direction, etc. is possible.

Since both sides of this dispute feed on the possibility of drawing an earth-shattering contrast between the two competing conceptual frameworks, I shall now spell out the details of the new analysis of scientific change in order to show that the usual polarized distinctions simply do not apply to operant theory.

I shall largely rely on Koertge's exposition. The result will be that operant theory must develop a "general correspondence relation" between itself and Goldman-type action theory if it is to supersede the latter. Specifically, the aspect of operant theory that pertains to the explanation of the occurrence of particular acts will (in all likelihood) have to allow for a role for internal states that is much like the one they play in Goldman's theory.

But, and this is the central point of using this analysis of scientific change, the envisaged operant theory plus

internal state theory need exhibit no more than a "general correspondence" or "strong analogy" to Goldman-type action theory. Although it will also be necessary to suggest how this alleged successor theory "corrects" and explains the workability of its predecessor, this will largely be an empirical matter.

The central idea of Koertge's theory of scientific change, which Koertge attributes to H. R. Post, is called the General Correspondence Principle (GCP, for short). The basic contention that the GCP expresses is that there is a general correspondence between any two successive theories in the history of science such that even in cases which involve dramatic changes in ontology, "there are, nevertheless, striking resemblances between the rejected theory and the new one which replaces it."⁵ Thus we can expect some, perhaps close, resemblances to obtain between action theory

and its putative operant successor but not necessarily either a fully conservative reduction or a total incommensurability. Koertge's statement of the GCP proceeds as follows:

If S and L are well-confirmed theories in the history of science and L is a successor of S, then an approximate or qualified version of some subset of the statements of S (call the subset S*) is derivable in L.⁶

Accordingly, there will be at most a difference in degree of approximation between, on the one hand, the replacement of the phlogiston by the oxygenation theory of combustion, and, on the other, the replacement of Newtonian mechanics by its successor. The effect on the dispute over the adequacy of operant theory is to pull the rug out from under the a priori stance adopted by both sides of the debate. However science proceeds in the end, there can be no a priori argument against attempts to synthesize the two conceptual frameworks of teleological action theory and the allegedly mechanistic operant theory.

Besides the general correspondence relation that is held to obtain between relatively complete successive theories, the GCP has an interesting claim to make about the way in which new theories develop so as to supplant their competitors. Thus, the GCP involves the following two-fold contention:

The General Correspondence Principle is the claim that nearly all successive theories in the history of science stand in a correspondence relation and that in those cases where there is no correspondence relation to begin with, the new theory will be developed in such a way that it comes more nearly into correspondence with the old.⁷

Now, as it will later emerge, this developmental aspect of the GCP is of the utmost significance for an assessment of the relation between operant theory and action theory. Operating with an inadequate analysis of scientific change according to which theories are static structures as opposed to dynamically evolving entities, both sides of the present dispute would tend to consider operant theory as decisively refuted if its account of animal behaviour could not be extended unrevised to cover human action as well. Yet if the GCP correctly characterizes the normal course of scientific change, then we should fully expect operant theory to develop in such a way that it would come into a general correspondence with action theory. If this is indeed the case, then action theory's continued adequacy to account for at least conscious, purposive action would not necessarily be tantamount to a denial of operant theory's ability to replace action theory. This would be so because it is of the nature of such theory replacement that predecessor theories, or revised versions of them, form a substantial part of their successors. However, it would have to be shown (1) that operant theory is more general than

action theory, (2) that many of the core statements of action theory constitute a subset S^* of operant theory (the successor L), and, (3) that operant theory can correct and explain the success of action theory. In other words the GCP predicts that a qualified or re-interpreted version of action theory should be adequate for at least some part of the domain it currently purports to embrace.

Obviously, a precise delimitation of S^* and L can be accomplished only with historical hindsight and only in formal terms. But the point of invoking the GCP is to support the argument that a highly revised version of operant theory could in a sense replace action theory, even if it is forced to preserve a great deal of its structure in a qualified form. I am only trying to show that such a conceptual change need not involve a radical categorial revolution and that, therefore, one need not invoke Feyerabend's extreme views nor those of a narrow positivism. On the positive side, the best that can be achieved at present is a speculative sketch of a plausible way in which such a theory change would move. This would be sufficient to take the sting out of the sharp distinctions, usually thought to be appropriate to this topic, between teleology and mechanism, action and movement, etc. The question I shall be attempting to answer in a somewhat speculative fashion is posed in a general form by Koertge:

Can one specify beforehand (i.e., without looking at L) which subset of the statements derivable in the old theory will be retained (in some corrected or qualified form) by the new theory?⁸

Koertge suggests a number of criteria which are inadequate singly, but which, when taken together, give us a rough idea of which part of an entrenched theory is most likely to survive largely intact across theory change. Her criteria are (1) the relative observational-theoretical character of concepts, (2) degree of universality of concepts, and (3) degree of confirmation. Now, unfortunately we do not have an experimentally tested general theory of human action that is also formally structured to a degree that is sufficient to allow a precise application of these criteria.

Accordingly, my strategy will consist simply in an attempt to show that a "great deal" of action-theoretic notions and distinctions either play a role within operant theory already or can plausibly be given such a role. For example, even Skinner admits that organisms behave at particular times much because of their current structure. And no action theorist would deny that there is some connection between such structural entities as wants and beliefs and overt behaviour. I shall therefore argue that some such states can sensibly be said to play a role in operant theory even though operant experimenters almost entirely refrain from referring to them. Such an exercise could serve two purposes. I intend it to show the futility

of attempting to assess operant theory armed with a set of diametrically opposed categories. I also expect my suggestions to have at least potential heuristic value for ongoing science itself. As I have previously mentioned, I reject the role of philosophy of science whereby it would be restricted to merely predicting which aspects of an entrenched theory are likely to survive scientific change. It must be emphasized therefore that the present inquiry is fundamentally concerned with its potential value for the theoretical articulation of operant theory itself. Clearly, from this standpoint, the present lack of formal structuring of both theories is no drawback whatsoever. On the contrary, conceptual comparison of action theory with operant theory along the lines suggested by the GCP should yield a plausible ordering of the two theories that is a necessary prerequisite to formalization of a possible successor theory. Fruitful theoretical innovations and potential experimentation may also be suggested by this further stage, although this is beyond the scope of the present work.

Already we know enough about the operation of the GCP to raise a sufficient number of questions to occupy this inquiry. Although a certain vagueness in these questions cannot be avoided prior to formalization, it must be kept in mind that it is this latter task that the present discussion is concerned to motivate. Among the questions

raised by the Post-Koertge analysis of theory change are these: In what sense is there a general correspondence between operant theory as it occurs in Skinner's writings and a current theory of human action such as that of Alvin Goldman? Given the sorts of expectancies the GCP gives us, are there some natural sorts of modifications or ways of developing operant theory so as to bring it into even closer correspondence with action theory? To what extent can action theoretic distinctions be drawn within the operant framework while still preserving much of the fundamental sense of both theories? What could it mean to say that operant theory "qualifies" or "reinterprets" action theory? Such qualification, according to Koertge:

...restricts the domain to which the S-theory statement truly applies and which specifies the degree of approximation to which the original statement is correct.⁹

The latter question above can thus be reformulated as follows: If a somewhat qualified version of an acceptable action theory is true, to what restricted domain might operant theory show it to apply? How is this domain related to the domain to which it does not apply? Why does it not apply to this latter domain? And why is it successful with regard to its restricted domain? Notice that all of these questions are meaningfully raisable only in the light of the analysis of theory change here adopted.

This fact raises general considerations that are worth mentioning since I am concerned to justify the use of this approach to the issues I want to discuss. It should be apparent therefore that which kinds of questions one tends to raise is a function of one's view of the nature of theory change. Moreover, we are still far from fully understanding this process. Such general questions as the following do not as yet admit of uncontroversial answers: What does it mean to qualify or reinterpret a theory? What does it mean to say that one concept approximates another? How does concept approximation differ, if at all, from theory approximation? Is "phlogiston" an approximation to "oxygen" over and above the phlogiston theory's approximating the oxygenation theory? Not necessarily it would seem; for those aspects of the phlogiston theory that survive theory change may make no essential use of the concept of phlogiston.

These questions are important for any investigation which purports to determine the sense in which action theory approximates operant theory by comparing the central concepts of the two theories. My main reason for raising these questions is simply to further support my contention that an understanding of the nature of scientific change is absolutely essential in order to so much as begin an assessment of such controversial new theories as Skinner's. For my purposes, because we are only beginning to study the nature of scientific change, I can do no more than

assume that we have an intuitive grasp of what an adequate analysis would look like. The most that can be accomplished at this stage is a plausible demonstration that such concepts as that of an operant very closely parallel in meaning the concept of an act. The intuitive grasp we have of scientific change should serve to remind us that the concept of an operant cannot be expected to express all that is expressed by the concept of an act. On the other hand, since successive theories overlap to a great extent, the mere fact that operant theory and action theory appear to be saying the same thing in certain respects does not necessarily imply that nothing but a trivial verbal transition is involved.

Another interesting general question that is of relevance for the present inquiry is the following: If such concepts as natural place and phlogiston actually are approximations to gravity and oxygen respectively, then what does it mean to say that there is no such thing as natural place, or no such thing as phlogiston? The subtle analyses of scientific change now emerging from recent efforts suggest that it is consistent to say both that the concept of x approximates the concept of y and that there are no x's. One simply has to say that oxygen has a number of properties previously attributed to phlogiston except at least one essential property such as, perhaps, that oxygen is an external agent in the process of combustion whereas

phlogiston is a product released in combustion.

Accordingly, we must look for a similar change in switching from the concept of an act to the concept of an operant. The effect that a total lack of any detailed analysis of conceptual change has had on critical discussions of behaviourism is easily demonstrated. It has been seriously suggested that proponents of behaviouristic theories must have to pretend to be anesthetized by virtue of their denial of the existence of consciousness and other specific mental states. But when the existence of heavenly bodies or natural place is denied, one is not thereby asserting that the sky is literally empty or that heavy objects do not tend to move toward the center of the earth when released from a position above its surface. This sort of confusion could be written off as just another instance of the confusion of meaning and reference, but this move hardly serves to provide us with a satisfactory analysis of theoretical change. Even when we discard the term "natural place" and claim it has no referent, we still want to maintain that gravity retains some properties previously attributed to natural place.

Thus the behaviourist's denial of the existence or causal relevance of mental states must be understood in a highly qualified manner. Whether the behaviourist employs different terms with which to characterize internal structure or not, he surely owes us some theory of the role

of internal states which corrects and explains the plausibility of the action theorist's account of the nature and causal role of mental states, events, and processes. As we shall see, it is in precisely this area that operant theory needs to be developed in order to come into a general correspondence with action theory that is closer than the one that currently obtains.

The foregoing brief sketch of theory change will suffice for the present. Particular points already mentioned will be stressed when I am actually comparing the two theoretical frameworks involved. I now want to ascertain the extent to which operant theory already closely parallels the structure of action theory. But before turning to this task it is necessary to show how operant theory bears out one of the predictions of the GCP. Recall that the GCP predicted that if successive theories in the history of science had no general correspondence between them in their early stages, that they would develop such a general correspondence. Accordingly, in order to show that operant theory has been moving in the direction of a general correspondence with action theory it is first necessary to sketch its early radical behaviourist origins. This task will occupy much of the following section.

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³ Ibid., p. 138.

⁴ Hamlyn, D. W., "Conditioning and Behaviour", in Borger and Cioffi (eds.), Explanation in the Behavioural Sciences, Cambridge University Press, 1970, p. 152 (emphasis added).

⁵ Koertge, op. cit., p. 170.

⁶ Ibid., p. 172.

⁷ Ibid., p. 176-177.

⁸ Ibid., p. 172.

⁹ Ibid., p. 176.

III. HISTORICAL DEVELOPMENT OF OPERANT THEORY OUT OF EXTREME BEHAVIOURISM

At this stage it might be asked why it is necessary to describe the structure of crude versions of early behaviourism. Why not simply contrast the most plausible interpretation of operant theory available with some theory of human action such as that of Goldman? The answer is that such a strategy would entirely undercut the evolutionary epistemology and evolutionary theory of scientific change that I am trying to develop. I am endeavouring to call attention to the gradual, developmental, character of science in direct opposition to the rather naive view whereby theories are envisaged as arising in a relatively complete form overnight and either completely confirmed or completely falsified.

This latter view is common perhaps because of our lack of study of the history of science until quite recently. Only completed theories have attracted the attention of philosophers in the past, partly because of a lack of historical perspective and partly because of an emphasis on the so-called context of justification. Throughout my argument I am opposing the widespread view of theories in general as disjunctions, only one of which can be true. And, in particular, the rigid disjunctive contrast between mechanism and teleology is here

rejected. But the only way to support my viewpoint is to attempt to bring out the entirely developmental character of science. It would thus be self-defeating to take some fairly complete interpretation of operant theory and compare it with another static theory. Besides the self-defeating character of such a strategy, the particular interpretation of operant theory I choose would be bound to appear arbitrary or ad hoc.

Accordingly, in order to dispose fully of the two views of reduction I attributed to Nagel and Feyerabend respectively, with their picture of reducing or replacing theories as static entities, it is necessary to emphasize the developmental character of scientific change. An exposition of early behaviourism and the ensuing sketch of operant theory's gradual divergence from it will best serve the foregoing aim and will also remove any air of arbitrariness accruing to the interpretation of operant theory I want to propose.

The aim of the present section is therefore to render fully transparent the very real extent to which early behaviourist theories were radically opposed to teleological, mentalistic theories. In showing operant theory's very gradual substantive divergence, I also want to emphasize its implicit abandonment of empiricism and its subsequent implicit support of an evolutionary philosophy. Only by seeing the thoroughly developmental character of theory

change can it be seen that the sense of "replacement" employed herein is closer to the sense of "synthesis" than "rejection".

Another theme of this section is to use a theory of scientific change to account fully for the current status of operant theory in Skinner's writings. In particular, it will be shown to be a natural feature of the gradual character of scientific change that Skinner has developed a theory which is fruitful at the core but which is nonetheless dogmatically placed in an obsolete framework. In order to refute the idea that whole theories emerge overnight it would thus be wrong to call attention solely to operant theory's divergence from early behaviourism.

Accordingly, I shall also emphasize the extent to which Skinner's positivistic outlook has survived over the last fifty years. This aim is not apologetic. In order to fully dispel a sense of arbitrariness to my interpretation of operant theory it is not sufficient to show how it is a gradual development from early behaviourism in accordance with the GCP's prediction. It is also necessary to justify the existence of inconsistencies between my interpretation and Skinner's current positivistic proclamations. In order, therefore, to fully remove a sense of ad hocness it is essential to see the naturalness of the inability of scientists to shake off all of their preconceptions.

A classic case of this aspect of theory change is the

Copernican revolution. Our lack of historical perspective results in a failure to see that Copernicus himself made only a very few dimly conceived innovations within an entirely Aristotelian metaphysical and cosmological framework. Our popular conception of him is that of one of the greatest revolutionaries in the history of science. Why? Because his dimly conceived innovations were subsequently developed--albeit over 150 years! -- into the Newtonian world view. We would not see the point of condemning Copernicus on the basis of inconsistencies within his system or between it and Newton's.

Yet because Skinner's current version of operant theory is placed in a positivistic framework we condemn the entire theory along with the positivistic declarations. The point is that we still have a lot to learn from studying the history of science and scientific change. My attempt to extract a valuable core of operant theory from both its early and its current behaviouristic foundations cannot but seem part of the natural process of scientific change. The idea that theories are complete from birth and that any revisions are ad hoc is totally misguided.

Consequently, I reject the widespread tacit opinion that we must either accept all of operant theory together with Skinner's positivistic biases or reject the entire research programme as being misguided throughout. The lesson to learn from the history of science is that we must acquire the ability to look for and pick out subtle shifts.

in theoretical foundations, to distinguish what is novel from the inessential and wrongheaded, at a more conscious level. The rationale for sketching early behaviourism, showing how operant theory has diverged from this framework, and pointing out the extent to which Skinner retains positivistic doctrines should now be sufficiently clear.

In my exposition of early behaviourism I shall refer mainly to the views of John B. Watson and Carnap. I shall attempt to make clear just which metaphysical, epistemological, and structural aspects of early behaviourism in psychology are left behind by operant theory and which features are retained by Skinner. Epistemologically, behaviourism in psychology was at one with behaviourism in philosophy. This affinity is clearly recognized by Carnap himself.

The position we are advocating here coincides in its broad outlines with the psychological movement known as "behaviourism"--when, that is, its epistemological principles rather than its special methods are considered. We have not linked our exposition with a statement of behaviourism since our only concern is with epistemological foundations while behaviourism is above all else interested in a specific method of research and in specific concept formations.

In this early statement of the Vienna Circle point of view, Carnap naturally meant to refer to the verifiability criterion of cognitive meaningfulness when speaking of his interest in epistemological principles. The positivist

declaration that only those psychological terms translatable into the physicalist observation language are verifiable and thereby meaningful could not fail to find acceptance among behaviourists in psychology. This is most certainly true of Watson as we shall see.

However, it is easy to show that a similar attitude pervades all of Skinner's writings. First, it is not implausible that Carnap had a direct influence on Skinner since Skinner lists Carnap's "The Unity of Science" in his references in his early The Behaviour of Organisms. Second, whatever the source of Skinner's positivism, it is clear that the translatability notion has never been abandoned by Skinner.

One still finds attempted translations of mentalistic statements into the behaviourist (observation) language even in Skinner's latest book, About Behaviourism, 1974.

On the other hand, there are subtle differences with regard to the epistemological function of such a translatability criterion. Consider Skinner's latest remarks on its role:

...there are perhaps no exact behavioural equivalents, certainly none with the overtones and context of the originals. To spend much time on exact redefinitions of consciousness, will, wishes, sublimation, and so on would be as unwise as for physicists to do the same for ether, phlogiston, or vis viva.²

In attempting to sort out similarities and differences between Skinner and Carnap it is again essential to refer to differing views on reduction or scientific change. One

thing Skinner has in common with Carnap and other logical behaviourists is a stubborn preference for an observation language. Thus a behaviourist observation language is the only one appropriate in psychology. But the type of theory of scientific change I attributed to Nagel underlies the logical behaviourist's thinking whereby only adequate theories are suitable candidates for reduction. The attempts by Carnap and other logical behaviourists to provide precise behavioural translations shows that they conceived the conceptual framework to-be-reduced as an adequate theory.

On the other hand, although Skinner employs the same analysis of reduction he views the to-be-reduced theory as false, as part of pre-science, to which only rough behavioural counterparts need be provided for mentalistic notions. There is thus a mixture of purely positivistic and non-positivistic doctrine in Skinner's view. There is the positivistic allegiance to observation language and the view somewhat akin to the GCP that it is not necessary to directly reduce an inadequate theory. That Skinner does not move much beyond the logical behaviourist's stance is clear from the fact that he does not explicitly recognize that psychological terms referring to internal structure could have structural as opposed to behavioural referents.

Another respect in which Skinner's behaviourism differs from that of the logical behaviourist can be seen with respect to the term "pain" as an example. The logical

behaviourist was concerned with our method of verification of someone else's being in pain. A description of pain behaviour exhausted the semantic content of the term for the logical behaviourist. But this was not Skinner's concern at all. For his purposes it was sufficient that one could conceive of pain as an internal stimulus so that he could explain how we learn to behave appropriately to private pain stimuli. Thus "pain" has an equivalent in Skinner's theory but nothing like semantic analysis is involved. For the logical behaviourist there never could be a distinction between pain and the public behaviour accompanying it. But for Skinner this distinction was allowable. It was sufficient that pain, such as that caused by a toothache, be accompanied by some observable event some of the time so that parents could teach their children to say such things as "I have a toothache". On Skinner's account, learning led the pain as stimulus to cause the verbal report in certain situations. The statement "I have a toothache" was never regarded by Skinner as a meaning analysis. As I have argued, therefore, we see in Skinner just what we should expect: the beginnings of a theory that could be interpreted non-positivistically, but one still clothed in positivist doctrines in his writings. Leave off the positivist restriction to observation language and internal pain stimuli can be ascribed any realist ontological status you like.

I shall now leave this contrast with Carnap and turn to Watson. Here again we will see epistemological, methodological, and metaphysical affinities. Substantive theoretical agreements will also be apparent. I shall first emphasize fundamental similarities in all of these respects, after which I shall turn to divergences. In About Behaviourism (Skinner's most recent book), after listing twenty standard criticisms to which he intends to reply, Skinner goes on to discuss Watson's place in the behaviourist tradition. The notable point is that Skinner does not once criticize Watson's philosophical outlook. Instead he criticizes Watson for attributing too much power to early behaviourist substantive theory. Watson was out of line simply because he made "some rather extreme claims about the potential of a newborn infant". But fortunately for the behaviourist programme, as Skinner sees it, "More than sixty years have passed since Watson issued his manifesto, and a great deal has happened since that time. The scientific analysis of behaviour has made dramatic progress..."³ This naturally suggests that Skinner finds Watson's general philosophic standpoint quite acceptable. Further evidence for this possibility as well as confirmation of agreement on substantive empirical questions is readily available.

In 1933, at which time Skinner had published some ten research reports (1930 to 1933), a book entitled Seven Psychologies by Edna Heidbreder was published. This

convenient survey of seven schools of psychology that were prominent in the first three decades of the century contains an excellent account of the behaviourist movement. What makes this source so useful is that the chapter on Behaviourism is devoted entirely to Watson with the exception of some brief discussion of the contributions of a few of Watson's followers. Rather than rely on Watson's own writings, this secondary source provides us with a picture that can be taken as reliably indicative of the general impression of behaviourism on the scientific community that Watson had created by 1930 when Skinner was beginning his own research. The highlights of the behaviourist platform that Heidebreder displays are particularly striking. Their amazing word for word similarity to the pronouncements Skinner has been issuing over the last four decades is quite impressive.

Referring to the first chapter of Watson's Behaviourism of 1925, Heidebreder communicates the attitude of the founder of behaviourism as follows:

If psychology is ever to become a science, it must follow the example of the physical sciences; it must become materialistic, mechanistic, deterministic, objective. To obtrude the mental is to make an opening for the mystical and the magical. Because behaviourism seeks to make psychology a science in the strictest sense of the term, it insists that the notion of mind be unequivocally discarded.

~~It is important to realize the vehemence and thoroughness with which the concept of consciousness is rejected.~~

Knowing the profound extent to which Skinner has carried this behaviourist banner through the ensuing decades, we realize that he must have found Carnap's "unity of Science" declarations delightful. The affinity between Carnap and Watson could hardly be closer what with Carnap's claim that all psychological statements were either translatable into the physicalist language or meaningless. The justification for rejecting the concept of consciousness was, as Heidebreder so nicely puts it, that "consciousness cannot be seen, nor touched, nor exhibited in a test-tube."⁵

Heidebreder portrays the full force of Watson's attitude and indeed the entire Carnap-Watson-Skinner outlook, so vividly that there is just no substitute for her own words. Continuing her account of the behaviourist rejection of the notion of consciousness, Heidebreder proceeds as follows:

Even if it exists it cannot be studied scientifically, because admittedly it is subject only to private inspection. Finally, a belief in the mental is allied to modes of thinking that are wholly incompatible with the ways of science. It is related to the religious, the mystical, and the metaphysical interpretations of the world. The notion of consciousness is the result of old wives' tales and monks' lore, of the teachings of medicine-men and priests. Consciousness is only another name for the soul of theology, and the attempts of the older psychology to make it seem anything else are utterly futile. To admit the mental into science is to open the door to the enemies of science--to subjectivism, supernaturalism, and tender-mindedness generally. With the simplicity and finality of the Last Judgement behaviourism divides

the sheep from the goats. On the right side are behaviourism and science and all its works; on the left are souls and superstition and a mistaken tradition; and the line of demarcation is clear and unmistakable.⁶

Seen against this radical background, the foundation for Skinner's pervasive attack on the explanatory fictional character of mentalistic concepts is all the more apparent. The latest statement of his unfailing allegiance to this programme reads as follows:

The mentalistic problem can be avoided by going directly to the prior physical causes while bypassing intermediate feelings or states of mind. The quickest way to do this is to confine oneself to...those facts which can be objectively observed in the behaviour of one person in its relation to his prior environmental history. If all linkages are lawful, nothing is lost by neglecting a supposed nonphysical link.⁷

The upshot is clear: Statements putatively referring to mental states either refer to directly observable intervening brain states or they do not refer at all. On the latter alternative there are no mental states and on the former they are mere links in causal chains. As I shall have a great deal to say later about the status and role of internal states and processes in operant theory, I shall simply draw attention here to Skinner's allegiance to the positivistic demand for direct observability and his emphasis on physicalism.

Other outstanding features of Watson's outlook shared by Skinner include the former's conviction that, in Heidebreder's words, "the methods of animal psychology might profitably be applied to human psychology..."⁸ Since I am attempting to reconstruct not only the structure of operant theory and its philosophical foundations, but also its methodological foundations, it is important to draw attention to the fact that a good deal of operant methodology may survive rejection of the theory's epistemological and metaphysical presuppositions. Specifically, so long as the above methodological principle does not carry with it an assumption of the total absence of significant differences between animals and humans there is little fault to be found with it.

On the contrary, I find Skinner's two arguments for this principle entirely cogent. The first point is simply that it is only by investigating "those features which animals and people have in common...can we be sure of what is uniquely human."⁹ Although there is no a priori reason to suppose that this is the sole legitimate way to discover what is distinctively human, it is surely a necessary supplement to any alternate approach which might contain the danger of exaggerating human and animal differences. The second argument Skinner offers for this methodological stance is also persuasive. Generally speaking, science moves "from simple to complex."¹⁰ It is no accident that the physical sciences progressed very

gradually from elementary statics and dynamics and simple observable astronomical motions to the complexities of the quantum domain only after theory and instrumentation developed sufficiently.

The psychologist, I believe, is in a unique methodological position in that, since we are intimately familiar with human behaviour in all its complexity, he must persistently and almost irrationally limit himself to studying simple behaviour first. This is not to say that all psychologists must avoid the study of complex human behaviour temporarily. Nonetheless I am convinced that it is essential for some to so restrict themselves. Besides the inherent danger of our failure to grasp the real differences that exist between humans and animals, there is the even more perverse tendency to concentrate on the study of human behaviour and use the results as a model for the explanation of animal behaviour. A certain balance between reducing men to rats and elevating rats into men is what is required. The common objection against all extrapolation from animals to man strikes me as every bit as question begging as was the protest against extrapolating from an earthbound science of mechanics to the explanation of "heavenly" motions that was so pervasive in Galileo's day.

There are further epistemological similarities between the outlook of Watson and that of Skinner. The aim of psychology for Watson was as follows according to Heidebreder:

"The general problem of psychology...is to predict and control behaviour."¹¹ More specifically:

...the task of psychology is to determine the stimuli that occasion any given response, and the responses occasioned by any given stimulus. Ideally the psychologist should understand the human animal as an engineer understands a machine.¹²

This engineering conception of the science of behaviour is so profoundly central to Skinner's methodological stance that some critics have gone so far as to argue that Skinner really has no theory of behaviour at all in any substantive sense. Operant "theory" is nothing but a technique or instrument of prediction and control. This distorted picture is certainly due to Skinner and Watson themselves by virtue of their uncritical adoption of the positivistic conception of science in general. Once this positivistic interpretation is discarded, however, there is no reason why the theory cannot be viewed realistically. Because of Skinner's positivistic emphasis on the engineering aspect of operant theory, it is entirely his own fault that critics such as Karl Pribram find the following judgement apt:

Science is knowledge; engineering is the application of that knowledge to human purpose. Skinner's interest is that of the engineer, and we are much in need of good engineering in our culture. But it is one thing to advocate good engineering and another to try to make it encompass all of science and human enterprise.¹³

Although there is undoubtedly a great deal of truth in Pribram's charge, it by no means follows that a substantive theory cannot be extracted from Skinner's writings which cannot be interpreted realistically. Originators of physical theories have presented theories in an operationist or instrumentalist framework without our thereby having to accept such an interpretation.

One point of substantive empirical agreement between Watson and Skinner deserves mention. As Heidbreder puts it, Watson's view of the nature of thought was such that:

Language is at first overt. By a process of conditioning, the child acquires words... [which]...give him the power of manipulating his environment without making the actual overt movements. This is all that is meant by thinking.¹⁴

Skinner asserts precisely the same view both in Verbal Behaviour and in About Behaviourism. In the latter he tells us that:

Covert behaviour has the advantage that we can act without committing ourselves; we can revoke the behaviour and try again if private consequences are not reinforcing.¹⁵

One wonders why critics of Skinner's attack on the explanatory value of mental states and events have failed to see through his polemical fog to the realization that in such passages as this, he is mentioning a process that looks remarkably like practical reasoning. This sort of point

will be made again and will support my argument that Skinner is only attacking a particular theory or characterization of the role of internal states and events, rather than banishing them altogether.

Watson and Skinner also concur on the nature of the acquisition of the tendency to think (in the inaudible sense). As Heidebreder describes it, Watson's view is that:

At first the child uses the overt movements of spoken language; then, largely because of his social environment, he learns to repress his overt speech...¹⁶

Skinner adopts the whole of this entirely speculative theory and simply gives it an operant wording. This suffices to show that Skinner's current, not to mention original, affinity to Watson's position goes beyond the epistemological and methodological aspects of behaviourism.

I shall now sketch the essential features of the substantive Watsonian theory originally adopted by Skinner. In contrasting the emerging operant theory substantively, I shall also call attention to its divergence from a mechanistic metaphysics and an empiricist epistemology. I shall also continually rely on the GCP and its associated evolutionary theory of scientific change; for I am convinced that the failure of the critics of behaviourism to appreciate fully the significant substantive differences between operant theory and Watsonian type behaviourisms is a direct result of a distorted vision by which theories are

perversely seen as static structures either true or false as they are originally presented. Accordingly, the developmental prediction of the GCP has a degree of relevance for what follows that cannot be overestimated.

When Watson first began promulgating the behaviourist philosophy of science he had no substantive theory of behaviour to speak of to go along with his philosophy. The discoveries of Pavlov came along at precisely the right time for the youthful behaviourist movement and they were adopted immediately by the proponents of behaviourism as though they were their own. The programme took on an empirical flavour in its adoption of its "hypothesis" that all behaviour, purposive and otherwise, could be assimilated to the (quasi-Kuhnian) classical conditioning paradigm. There was, and still is, no doubting the mechanistic character of this theory. For convenience of reference it might be appropriate to enumerate some of the essential features of Watson's theory so as to render fully evident the extent to which operant theory diverges from it.

(1) According to this version of behaviourism, all behaviour was of a reflex nature by virtue of its fitting the S-R formula. Recall the problem of psychology as Watson envisaged it: Given the stimulus, an investigator was to discover the response and vice versa. Stimuli were regarded as straightforward efficient causes and there was

one for every conceivable response. Here is a mechanistic feature.

(2) Learning was also mechanistically conceived.

Remarkably enough there could be no really novel behaviour on this theory. There could be novel reflexes, i.e. S-R connections by virtue of the possibility of novel stimuli becoming associated with existing responses. All learning was classical conditioning; it consisted in the pairing of unconditioned stimuli with the stimulus of the innate S-R connection. Learning occurred when the new unconditioned stimulus came to elicit the original response, or one very like it, without the occurrence of the original stimulus. This is also clearly mechanistic. But even more interesting is the fact that here we have a behaviourist analogue of the empiricist account of learning via the association of ideas, only now it is stimuli that are associated. The affinity with empiricist epistemology is thus clear. Watson's favourite example of this learning was an experiment in which a child learned to fear a pet such as a rabbit or a cat. The innate reflex in this case is a startle response consisting of jumping and crying. The pet is the unconditioned stimulus and is presented immediately after which there occurs a sharp loud noise behind the child's head. After a few pairings "associations", of the pet and the noise, the pet alone comes to elicit the startle response. All learning was believed to be of this form. Stimulus

generalization could also be invoked. Similar animals in the above experiment, including stuffed animals, elicited a similar response.

These two central features of Watson's theory should suffice for present purposes. It is easy to see that Skinner adopted this theory and philosophy of science wholesale. His dissertation included, as a major part, a historical account of the emergence to prominence in psychology of the concept of a reflex from Descartes to the early twentieth century. Now Skinner's publications from 1930 to 1938 consist of a fascinating document of the gradual isolation of a new paradigm: the distinction between reflex or, as he called it, respondent behaviour and operant behaviour. Although he seems to be in ignorance of Thorndike's Law of Effect, the new operant framework is just this notion expressed in conditioning terminology. Consider the following differences between operant and respondent theory:

(1) Whereas there are two terms to the S-R formula, Skinner's concept, contingency of reinforcement, has three terms, S, R, and reinforcement. The latter refers to events produced by or contingent upon R in the presence of S. Thus S is not a stimulus which elicits R innately. It is only an event or object which may only happen to be present on the occasion of a response's being followed by reinforcement. By virtue of being paired with referents of the other two terms

it becomes a discriminative stimulus and acquires the power to increase the likelihood of R on future occasions. There is no one-one stimulus-response relation in operant theory mainly because of intermittent reinforcement. On schedules of reinforcement a single discriminative stimulus can control a steady pattern of responding. This is one major theoretical divergence from both classical conditioning and Thorndike's theory. Since operant behaviour is emitted and controlled by its consequences, rather than elicited as is respondent behaviour, it does not have the mechanistic character of the latter. Indeed the centrality of consequences of behaviour in operant theory is one of the fundamental reasons why the theory is much closer to a teleological view than is Watson's. For this reason, the sharp contrast between mechanism and teleology simply fails to do justice to the differences between Watson and Skinner.

(2) Learning is not conceived in narrowly mechanistic terms. (Learning according to operant theory is as quasi-teleological as is evolution by natural selection and for a very good reason: There is a remarkable correspondence between operant conditioning and natural selection. In sharp contrast with respondent conditioning, novel behaviour is the stock and trade of operant theory. In the former, new stimuli are associated with old. In the latter, new forms of behaviour are generated out of a given set of

variations by a process called shaping: the differential reinforcement of successive approximations to a final form. This process is like trial and error learning except for the recognition in operant theory of the role of sets of variations in response types as opposed to discrete trials, not to mention the role of positive consequences as well as "error". It seems that for both operant conditioning and natural selection to occur there must be a set of variations in response types and species respectively. Reinforcement selects certain variations in response types; natural selection, presumably the consequences of the behaviour of species, selects certain species and not others. It is hard to see how natural selection accounts for the original, and continued, variation. Perhaps in this case operant conditioning can serve as a model for natural selection. For, since reinforcement in the natural environment is never very precise, it is always further sets of variations that are differentially selected rather than rigid unvarying response types. Such rigidity can be produced naturally and especially experimentally. But because fairly wide variations in a single type are usually also reinforced, continued variation--essential for increasing novelty--is maintained.

Because of the similarity between operant conditioning and evolution by natural selection, the label "mechanistic" is at best unilluminating. So is "teleological" for that

matter; a new metaphysics is in order. Furthermore, and I shall no more than allude to this possibility, if an epistemology can be modelled on evolution by natural selection then it can be informed by the nature of the operant conditioning process as well. How such an epistemology would differ from that of traditional empiricism is a question beyond the scope of the present inquiry which is simply concerned with the relevance of operant theory to the explanation of behaviour.

I have now sketched some of the theoretical, epistemological, metaphysical, and semantical differences between operant theory and other behaviourisms. Much more detail could be provided in the way of showing the painfully slow theoretical change in Skinner's writings. For example, Skinner still employed the concept of the reflex in discussing both types of conditioning in his 1938 The Behaviour of Organisms, even after discovering intermittent reinforcement by 1937. I have given other sorts of evidence of Skinner's retention of philosophical and methodological aspects of early behaviourism. All of this is in line with the GCP and its associated view of scientific change. Some similarities between operant theory and action theory are now evident, especially the emphasis within both frameworks on the consequences of behaviour. Further correspondences will be shown, and still others proposed, in the next section.

Before leaving Carnap and other logical behaviourists it is important to mention one more point of similarity and difference. It seems clear that Carnap was more concerned with the translatability or reduction of psychological terms into the physicalist language than he was with the more extreme empiricist demand that they be translatable into sentences about behaviour. This is evidenced by Carnap's willingness to abandon this latter thesis while retaining physicalism. In his 1957 comments appended to his early "Psychology in Physical Language", he revises his view saying that now, as far as psychological concepts are concerned, "it seems to me more in line with the actual procedure of scientists, to introduce them not as disposition concepts, but rather as theoretical concepts."¹⁷ He then refers the reader to Feigl's writings on the identity theory for the "present conception of physicalism."

On the other hand, logical behaviourists such as Ayer and Ryle were unwilling to give up the narrower empiricism whereby a dispositional analysis of mentalistic terms and statements exhausts their content. Psychological terms such as "anxiety" do not refer to internal states on this view but simply to behaviour or dispositions to behave.

Now, as I shall argue, operant theory is, already in Skinner's writings, closer to Carnap's empiricism than it is to that of Ayer and Ryle. Although it is extremely unclear in Skinner's writings, there is a definite realist place for

structural states, events, and processes in his version of operant theory. There is thus no reason to restrict the semantic content of psychological state terms to summaries of behavioural dispositions. This, again, is not a biographical aside. I emphasize it solely to further show that the gradual change in operant theory really is such as to make the resulting transition an instance of scientific change as Koertge describes it.

The foregoing comments nicely set the stage for further attempts to show the existence of a general correspondence between operant and action theory. The GCP will prove invaluable in sorting out the apparent contradiction between my assertion of the place of structural states in operant theory and Skinner's never ending polemic on the status of mental entities as explanatory fictions. Accordingly, I shall begin the following section with this issue prior to a detailed comparison of operant and action theory.

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³ Ibid., p. 7.

⁴ Heidbreder, Edna, Seven Psychologies, Appleton-Century-Crofts, 1933, p. 235.

⁵ Ibid., p. 235.

⁶ Ibid., p. 235-236.

⁷ Skinner, op. cit., p. 13, emphasis added.

⁸ Heidbreder, op. cit., p. 240.

⁹ Skinner, op. cit., p. 226-227.

¹⁰ Ibid., p. 227.

¹¹ Heidbreder, op. cit., p. 246.

¹² Ibid., p. 247.

¹³ Pribram, Karl, "Operant Behaviourism: Fad, Fact-ory and Fantasy?", in H. Wheeler (ed.), Beyond the Punitive Society, W. H. Freeman and Co., 1973, p. 112.

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14 Heidbreder, op. cit., p. 250-251.

15 Skinner, op. cit., p. 103.

16 Heidbreder, op. cit., p. 251.

17 Carnap, op. cit., p. 197.

IV. COMPARISON OF THE STRUCTURE OF OPERANT AND ACTION THEORY

A. According to the General Correspondence Principle, if L (operant theory) is a successor of S (action theory) then an approximate or qualified version of some subset of the statements of S (call the subset S*) is derivable in L. Now in order to allow the derivation of statements pertaining to the relation between an organism's internal states and its behaviour, operant theory must develop in such a way as to include a place for structural entities. There are two strategies one might adopt at this point.

One approach would be to first recognize that much of Skinner's polemic can be dismissed along with positivism so that we might as well forget about it, and then combine some of the insight of operant theory with a theory of action such as Goldman's. But this approach would fail to show fully how the proposed theory would have been developed in a way that is natural according to our analysis of scientific change. That is, besides proposing a theory of human action that may stand or fall on its own merits, I want to propose a theory that supports the evolutionary theory of scientific change I am employing. By illustrating the gradual developmental character of the proposed theory

I accomplish two things: (1) obtain support from Koertge's theory of scientific change (support which is thus independent of whatever other merit the theory might possess); and (2) help to establish the evolutionary picture of science that is vaguely suggested by Koertge's analysis. Moreover, if I did not continue to attempt to sort out valuable from obsolete elements in Skinner's own writings, much of my contention regarding the way in which such divergent static frameworks as those of mechanism and teleology can gradually develop into a synthesis would lose its force.

Thus it will not do to simply reject Skinner's arguments against mental entities and adopt some such view as Goldman's wholesale. The way to use the GCP properly is to show that, just as I have been arguing, Skinner's arguments are a mixture of obsolete positivist polemic and insight into a new theory. It is to a large extent because the developmental aspect of operant theory and the similar developmental character of scientific change are mutually supporting that I cannot abandon this approach in favour of the static construction of a theory of action.

My strategy in what follows, therefore, will be to show how Skinner's arguments can be easily dismissed if interpreted positivistically and how, if interpreted as being a switch in emphasis from interest in act-token occurrences to act-type development, there is some point to them. Since this is not meant to be of biographical

interest, I am not concerned with whether Skinner would agree with my reading of his arguments. Since I also want to support my claim that critics of Skinner are blinded by their lack of an adequate analysis of scientific change, it will be most convenient to discuss Skinner's arguments against mental entities by replying to recent criticisms of these arguments.

Russell Keat has very recently attempted to separate out the various ways in which Skinner attempts to establish the contention that mental entities have no explanatory power vis a vis behaviour. As might be expected, Keat finds Skinner's arguments unconvincing at best. The first of five such arguments offered by Skinner that Keat discusses involves what Skinner calls "unfinished causal sequences". As I shall have much to say on the positive side about this particular type of argument, I shall leave it until the last.

I shall begin my discussion of this issue with the argument against reference to mental entities in explaining behaviour whereby, in Keat's words, "mentalistic explanations are defective because they fail to provide us with the means of predicting and controlling behaviour."¹ Now there is a positivistic and a non-positivistic interpretation to this argument depending on what one considers to be the explanandum of operant theory. Suppose the explanandum is taken to be the occurrence of particular

events, act-tokens, at particular times. Naturally one will have to refer to current structure as a substantial part of one's explanans. In this case it is difficult to imagine acceptable non-positivistic arguments against reference to current internal states for the purpose of prediction and control.

But the case where the explanandum is the generation or change of act-types is not so clear. One could argue that even here it makes sense to say, as Keat does, that in order to control behaviour in the sense of changing act-types we can best proceed by reasoning with agents so as to get them to change their beliefs or attitudes. But if Skinner's theory is that such internal states are changed together with act-types and that both are altered by the alteration of contingencies of reinforcement, then it is a straightforward logical point that one must refer to reinforcement contingency changes in order to explain or control both internal state changes and act-type changes. Whatever the merits of this theory, the objection against mental entities is surely not founded on positivistic doctrines. I shall construct an elaborate analogy in order to further support this point later. For now I simply want to survey Skinner's arguments for the explanatory fictional character of mental entities in order to show that there are these two dimensions to them.

The first dimension is positivistic; the second suggests a particular theory of act-type generation and emphasizes this process as opposed to act-token occurrence. Actually there is still a third dimension, one in which the general correspondence principle comes into play. In attacking the role of mental entities, states, and events, Skinner is not thereby talking about an empty organism as some critics have absurdly believed. As far as applying the GCP to operant theory is concerned, we can interpret Skinner as attacking only a particular theory of the role of internal states. We can even leave open the ontological realization of such states. Thus, ignoring Skinner's reliance on observation, we can argue that Skinner's view is, as it should be, an approximation to the interpretation I am proposing:

In a more advanced account of a behaving organism "historical" variables will be replaced by "causal". When we can observe the momentary state of an organism, we shall be able to use it instead of the history responsible for it in predicting behaviour. When we can generate or change a state directly, we shall be able to use it to control behaviour.²

Without an adequate analysis of scientific change in hand, we cannot help but get the impression that Skinner is giving us something with one hand and taking it back with the other. He appears to deny the existence of structural states as well as their explanatory power only to talk of

their status positively in other places. However, this confusion is neatly dissipated by the account of scientific change endorsed earlier. For example, the Galilean says: "There are no heavenly bodies," and appears to the Aristotelian to be asserting that the sky is literally empty, while in actual fact the Galilean is simply introducing a new theoretical framework for dealing with bodies in space. Analogously, much of Skinner's polemic against mental entities can be seen as the claim that traditional theories referring to mental entities in the explanation of behaviour constitute approximations to an adequate theory -- and this is independent of how Skinner himself views the matter. Just as the Galilean replaced the Aristotelian framework along with a different conception of its entities, so we can view operant theory as replacing, or calling for the replacement of, a particular theory of the status and function of internal states and events.

As far as I can see Keat's failure to appreciate these points must stem from an inadequate view not only of theoretical change but of the very structure of theories as well. I do not see any other way to make sense out of Skinner's dismissal of mental entities on one hand and his claims such as the following, on the other: "An organism behaves as it does because of its current structure..."³

Since I am concerned to emphasize the gradual character of scientific change, as well as to reconstruct a

plausible interpretation of operant theory, it is important to call attention to irreparable arguments as well. Accordingly, another of Skinner's arguments against mental explainers is, in Keat's words, "that the very behaviour we wish to explain provides our only means of identifying the presence of the 'explanatory' mental items."⁴ Keat is right to reject this argument because there is surely no good reason to suppose that psychology is any worse off than any other science when it comes to the justification of the postulation of theoretical entities.

Many of Skinner's arguments against referring to mental entities or states in the explanation of behaviour have an objectionable a priori and positivistic character. But once we see that Skinner does believe there is a role for internal states and that the apparent inconsistency examined above can be dismissed by referring to the GCP, it becomes evident that Skinner has a reasonable methodological argument against mentalism as well. This argument is directed toward establishing the possibility of a quasi-phenomenological science prior to investigating underlying structure. I say "quasi" because it would be a mistake to view operant theory on the analogy of phenomenological thermodynamics rather than the theory of evolution by natural selection. This argument complements an earlier discussed argument. Generally in science phenomenological theories can be developed independently and often serve as a guide to

structural theorizing; thus, a phenomenological science of behaviour should be at least possible. Moreover, if our phenomenological theory is also, if not primarily, a theory purporting to account for the acquisition of internal structure, then we have all the more reason to avoid internal structure for the present and confine ourselves to attempting to develop a purely phenomenological theory. Now there is surely no a priori or positivistic overtone to this methodological suggestion. I do not mean to suggest that this is actually Skinner's view, but as such it is a reasonable component in the methodological foundations of operant theory. If so, then clearly operant theorists are not committed to some form of fictionalism simply by arguing for the possibility of a prior phenomenological theory. Surely this is the sense of the following assertion of Skinner's:

...we do not need to try to discover what personalities, states of mind, feelings, traits of character, plans, purposes, intentions, ...really are in order to get on with a scientific analysis of behaviour.⁵

The point is nothing stronger than that it is possible to investigate behaviour without first constructing a theory of internal states, entities, and processes. The claim is not uncontentious, but neither is it tantamount to a denial of ever needing to talk of internal structure. Operant theory can be regarded as being analogous to Newton's

méchanics. Once completed it can be reduced in generality to account for specific domains. Operant theory is like the theory of evolution in being applicable to all species. It clearly has to be enriched to account for human behaviour, just as Newton's theory has to be enriched to subsume fluid dynamics.

I can find no other isolable arguments against the postulation of mental or otherwise structural entities in Skinner's writings that admit of a worthwhile reconstruction. There surely are irreparable arguments, such as Skinner's attack on the notion of an inner agent. Skinner's argument seems to be that mentalistic explanations take the form of explaining the behaviour of human agents by ascribing properties to an unexplained inner agent. But Keat's reply to this argument is entirely correct; as he argues, it is,

...perfectly proper to accept the use of mentalistic explanations, and the concept of human agency, without thereby being committed to either a defense of human autonomy, or a belief in "inner agents".⁶

Moreover, even if Skinner's argument can be viewed as a legitimate form of the criticism of the version of the agency theory whereby acts of volition apparently launch an infinite regress, it still does not follow that other mentalistic theories are ruled out.

It is now time to consider the first of Skinner's objections to mentalism that Keat discusses. This argument,

like some of the others can be interpreted favourably even though it may not really be Skinner's own view. Thus it is difficult to view Skinner's point about "unfinished causal sequences" as anything but the launching of an infinite regress that would render all scientific explanation impossible.

One of the many instances in Skinner's writings of this argument is reproduced by Keat as follows:

We may object, first, to the predilection for unfinished causal sequences. A disturbance in behaviour is not explained by relating it to a felt anxiety until the anxiety has itself been explained. An action is not explained by attributing it to expectations, until the expectations have in turn been accounted for.⁷

It is obvious why critics have seen this argument as launching an infinite regress rendering all explanation impossible. And even if it is granted that the anxiety itself must be explained, the explanatory power of anxiety and other internal states is not thereby eliminated. There is clearly a difficulty in seeing such an argument as being anything other than confusion.

Nonetheless, I shall attempt to reconstruct an argument that at least bears a resemblance to Skinner's argument and which is not so easily rejected. The argument I shall offer will not show that particular actions cannot be explained by reference to internal states. On the contrary, my central aim in this section is to show

that operant theory already has begun to develop in this direction and that it must continue to do so in order to come into a general correspondence with theories of human action. Rather, I shall develop an argument which shows that, regardless of what Skinner is attempting to show, we can view him as suggesting merely a switch in emphasis to a dynamic picture of evolving behaviour-types and internal structure. The central focus of operant theory is thus upon explaining the acquisition of both act-types and internal structure. The problem of explaining particular act occurrences in terms of current structures is simply less interesting than it is in physical science where we are not dealing with rapidly evolving structures.

I shall develop an analogy in order to further clarify this point. But first I want to comment on another passage of Skinner's in order to set the stage for my argument. In trying to sort out Skinner's stand on mental entities, Keat considers the possibility that Skinner regards them as mere by-products of conditioning, brought into existence by reinforcement histories, but playing no causal role vis-a-vis the occurrence of particular actions. As evidence for this view, Keat cites the following passage from Skinner:

What is felt when a person protests is usually called resentment,...but we do not protest because we feel resentful. We both protest and feel resentful because we have been deprived of the change to be admired or receive credit.⁸

Now if such internal states as resentment were mere by-products of conditioning history, then Skinner would be quite clearly contradicting himself when he says such things as that an organism behaves as it does because of its current structure. Among other things there is at least a confusion between act-types and act-tokens here. It seems clear to me that Skinner wants to say that the occurrence of act-tokens is to be explained largely by reference to internal structure while remaining uncommitted on the particular sort of theory that might best account for the status and role of internal states. It can also be maintained that Skinner is primarily concerned with explaining the quasi-evolutionary generation of act-types. Thus in the above passage he must be regarding "protest" as the name for an act-type, in which case the claim is that since we acquire act-types such as that of protesting together with the acquisition of feelings of resentment, the latter can hardly serve to explain the former. Both events, the acquisition of the act-type and the acquisition of the internal state constitute a single explanandum. Now the point is this: Regardless of the adequacy of this theory, it is clear that there is an interpretation of it which avoids both the "unfinished causal sequence" absurdity and the "by-product" nonsense.

More precisely, the argument as I have reconstructed it becomes this: If the event which is S's acquisition of

an internal state such as, for example, that representing an ability to do A at t is identical with the event which is S's acquisition of the act-type of doing A at t, then one cannot invoke this ability to do A at t to explain the acquisition of the act-type of doing A at t. Notice that this does not, as indeed it should not, exclude the possibility of using current abilities (current structure) to explain the occurrence of tokens of A at t. For instance, when we explain the acquisition of the act-type of driving a car for some agent S, we are at the same time explaining the acquisition of the ability. The explanandum event is the same in both cases. Employing the distinction between basic acts (physical movements) and non-basic acts (acts, such as flipping a switch, done by moving physically) we arrive at the following:

- (1) S has the unacquired ability at t to do A at t iff A is a basic act-type for S at t.
- (2) S has the acquired ability at t to do A at t iff A is a non-basic act-type for S at t.

From these propositions it follows that operant theory, and any adequate theory for that matter, must admit the existence of unacquired or unlearned abilities and thereby whatever structural representation underlies them. That is organisms do not have to learn (acquire the ability) to move

their limbs, at least not normally. Our interest here is in acquired abilities and again some form of structural representation must be admitted. With respect to these abilities we can also say such things as the following: The acquisition of the ability to turn on a light by flipping a switch is identical with the acquisition of the non-basic act-type of flipping a switch by moving an arm in a certain way. Here again there is no denial of the causal efficacy of abilities vis-a-vis tokens of act-types.

Compare this interpretation with such passages from Skinner as that involving resentment and also with the following statement of Skinner's: "A single set of facts is described by the two statements: 'He plays well.' and 'He has musical ability'."⁹

I clearly cannot claim that Skinner has my interpretation of operant theory in mind, for this statement is dangerously close to the logical behaviourist theses according to which reference to such a "single set of facts" is designed to avoid commitment to structural states. I simply want to suggest that by regarding operant theory as an emerging account of the developmental generation of act-types, and internal states, we can view Skinner's own comments as just the collection of confused hints that the GCP says we should expect.

By interpreting certain passages as pertaining to the acquisition of act-types I have reconstructed an interpretation of the theory which avoids the unfinished causal

sequence argument. But once again, since I want to fully demonstrate the necessity of adopting a theory of scientific change whereby we are instructed to sort out the valuable from the confused as opposed to adopting a naive falsificationism, it is important to see that there are passages that do not admit of such an analysis. (Knowing that Skinner uses the term "operant" as I have been using "act-type", and "response" as I have been using "act-token", it seems impossible to avoid regarding the following as anything but an instance of the "unfinished causal sequence" argument. Referring to future neurophysiology Skinner says that when we have our completed account of behaving organisms:

We shall know the precise neurological conditions which immediately precede say, the response, "No, thank you." These events in turn will be found to be preceded by other neurological events, and these in turn by others. This series will lead us back to events outside the nervous system and eventually, outside the organism.¹⁰

Skinner could quite clearly not invoke his "unfinished causal sequence" argument to reject our decision to explain such responses in terms of internal structure. Since Skinner does not appear to be talking about tracing the causal sequence backward through the agent's reinforcement history but rather just to temporally proximate environmental stimuli, we also have here a mixture of the older behaviourism whereby a separate stimulating event is required for every response.

I shall now wrap up the foregoing comments with the analogy I have been alluding to. It will serve to clarify the difference between explaining event-token occurrences and event-type change. It should be emphasized first, however, that I have two themes going at once here, corresponding to the two aspects of operant theory. One aspect pertains to the explanation of act-token occurrences and is, I am arguing, the less significant of the two.

The foregoing discussion of the role of internal states was designed to show that, in accordance with the GCP, this aspect of operant theory must develop so as to include a sizable role for internal structure. How particular structural concepts mesh with operant and action theoretic concepts will be dealt with later.

The other theme, involving the developmental aspect of operant theory is to provide a rationale for a somewhat disinterested attitude toward explaining act-token occurrences with the result that much of the interest in operant theory should focus on its account of act-type generation. Now this theme leads to such questions as these: How does the explanation of such a process differ from explanatory patterns in the natural sciences? And what is the force of Skinner's claim regarding the adequacy of "functional" laws for a science of behaviour? If there is a role for internal structure vis-a-vis act-token occurrences, then we should expect to find a place for some theoretical laws. How would they relate to the

putative functional laws Skinner is talking about? It is easy to see how these questions relate to the "unfinished causal sequence" confusion. Since there is the older behaviourist element to this confusion whereby there is alleged to be direct S-R connections, with mere physiological mediation, it has been natural for the critics of all forms of behaviourism to suppose that Skinner is talking about the possibility of establishing functional laws relating currently impinging stimuli directly to act-types. If this were indeed the case, then given the occurrence of stimuli of the type mentioned on one side of the functional relation, we could predict responses (act-tokens) of the type mentioned on the other side of the functional relation. Thus a "finished" causal sequence, would have to be a lawful S-R connection.

Whether this is actually Skinner's view or not, I hope it will be abundantly evident that, according to the interpretation I am proposing, there is no commitment to the possibility of such laws. Just as I re-interpreted the call for a "finished causal sequence" to be a call for an emphasis on relating history of reinforcement to act-type acquisition, so I shall re-interpret the "functional law" talk to pertain to this aspect of the theory. But even here there will not be any functional laws mentioning particular reinforcement histories and particular act-type acquisitions. Rather, I

shall suggest the possibility of a general law obtaining between any reinforcement contingency and any act-type generation.

Consider, finally, the following analogy. Imagine a sheet of plate glass to be analogous to an organism. The breaking of the glass on a specific occasion would be comparable to an act-token occurrence; a range of possible forces or impacts would be analogous to currently impinging stimuli. We can say of the sheet of plate glass that it has the event-type of breaking under a certain minimum impact x . This corresponds to an organism's possession of an act-type. Already there are significant disanalogies. In the case of glass there will be a functional relation between "impinging stimuli" (impact x) and "behaviour" (breaking). Operant theory, as I view it, is not committed to such relations in the case of organisms. In the latter case, if there are any general relations at all, they will obtain between internal structure and overt behaviour. This pattern of explanation is relatively analogous to explaining impact-breaking functional relations in terms of underlying theoretical structure. Both patterns of explanation are similar to the explanation of thermodynamical events in terms of statistical mechanics. The difference is simply that in the case of organisms, the "behavioural regularities" obtain between internal states or events and behaviour and not between the latter and immediately

preceding stimuli. It is no wonder that action theorists primarily concerned with such a pattern of explanation have looked to theoretical laws connecting internal structure with behaviour while finding entirely mysterious the claim that functional laws relating current stimuli and behaviour could replace them.

But what does one reply to the following question : Why does our sheet of plate glass have just this event-type and not one whereby the minimal sufficient impact required to break the glass is y, or z, or some other force? Obviously it would be explanatorily empty to answer this question by referring to the current structure of the glass! I submit that it is precisely in this sense that Skinner has, albeit confusedly, tried to label mental entities as explanatory fictions. Clearly the answer to the question of why the glass behaves as it does would also be an answer to why it has the current structure it has.

In order to obtain an appropriate reply to the above question, imagine the following hypothetical experimental situation. Suppose that we can alter the molecular structure of the glass by passing X-rays of a certain intensity through it. Thus ~~our~~ question might be answered by a reply to the effect that X-rays of a particular intensity were passed through the glass and because the X-rays were of this specific intensity, the glass has event-type x rather than y or z. Furthermore, let us

imagine that there is a functional law, somewhat like Boyle's, such that there is a direct functional relationship between changes in the intensity of the X-rays and changes in the event-type possessed by the glass. Notice that there is little room for confusion here because at the level of explaining particular "responses" we are talking about impact, whereas in explaining "act-type" changes we are referring to X-rays. Unfortunately this is precisely a very likely source of confusion for interpreters of operant theory since the same reinforcement terminology is used for both act-token occurrences and act-type acquisition.

In any case, we have uncovered a sense in which there could be a purely functional law accounting for the behaviour of the glass which is not itself explainable by reference to the microstructure of the glass (at least not that microstructure directly responsible for the glass's breaking). It is noteworthy that with such a law in hand, and given initial conditions to the effect that specific X-rays had impinged on the glass, we could deduce a prediction of the glass's behaviour, without mentioning its microstructure or theoretical laws connecting it to behaviour. But it would also be true that our deductive schema could be filled out so as to include such theoretical laws. In the case of organisms, therefore, operant theory expects there to be a general functional law of reinforcement.

ment sufficient to explain all act-type changes. But we will still want a theory of the relation between acquired internal structure and act-token occurrences. How such structure will be conceived differently from the way it is revealed in introspection is an open empirical question. But surely the above analogy is one reasonable way of making sense out of the following statement of Skinner's:

The physiologist of the future will tell us all that is happening inside the behaving or organism. His account will be an important advance over a behavioural analysis.

There is another important disanalogy between plate glass and organisms, according to operant theory, that I want to emphasize. Our theory of event-type changes in glass is mechanistic in two senses. X-ray bombardings are straightforward efficient causes. Second, there need not be any really novel structural changes or novel event-types produced. Very mechanical alteration would result, say, if something like changes in elasticity were the sole result of such X-ray bombardings. I have been concerned to reject both of these mechanistic features. The "functional law of reinforcement" can only work if there is a set of variations of a number of act-types already occurring. Reinforcement does not primarily operate, if at all, directly upon internal structure; it is consequent upon act-token occurrences. As such it can only select from the range of

variations already occurring: "only" in the sense of not being responsible for the very existence of some such occurring variations. But actually it can also generate novel behaviour as well. Reinforcement can generate new variations because there is always a range of variations around some median point. For example, if a rat is pressing a lever over a period of time there will be some average pressure on the lever exerted. But there will also be a range of such pressures, so that if reinforcement is suddenly contingent upon pressures far above average, but still within the previously occurring range, then the entire range will shift upward. Novel forces which have never been reinforced before will result. The same is true of the "shaping" of complex act-types. Successive approximations are reinforced, but this is only possible because of the continued maintenance of quasi-trial and error ranges of variations. Thus operant theory accounts for novel act-types by reference to act-consequences and is, thereby, not narrowly mechanistic. Moreover, in the case of the generation and change of act-types we are talking about a cumulative evolutionary-like process as opposed to mere change such as is typified by mechanical re-arrangement.

Returning to the main thread of the argument, it is now imperative to tie some of the foregoing threads together. I have employed an analogy according to which

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it would be possible to have a functional law relating X-ray changes to the event-type changes of glass. I pointed out that the counterpart functional law pertaining to the act-type acquisition of organisms was disanalogous in at least the respect that the independent variables of the latter operated, not directly on internal structure as in the case of glass, but rather as events consequent on act-tokens. I then suggested a sense in which this fact made the act-type acquisition account less mechanistic than the explanation of the event-type changes of substances such as glass. Prior to that, it emerged that the explanation of events, and act-token occurrences was similar in both requiring reference to theoretical laws connecting internal structure to event-token occurrences. I cited Skinner's allusion to future neurophysiology for evidence that he recognizes the necessity of eventually filling in the picture in this way. Accordingly, the suggestion becomes that X-ray impingings account for internal structure of glass and its possession of event-types. What is needed is a theoretical account of how the same X-ray intensity regularly produces the same event-type. And for this part of the story we need theoretical laws relating X-rays, internal state changes, and overt behaviour. Similarly, for Skinner, the explanation of the behaviour of organisms is to proceed in the same manner. Methodologically, it is possible to search for such a functional law without denying

the eventual necessity of a structural account to fill in the gap. But for many critics of behaviouristic accounts of human behaviour, the above analogy will beg one of the most fundamental questions at issue.

The question apparently begged is this: Are all act-token occurrences entirely determined by history of reinforcement generated internal structure just as all event-tokens of breaking of glass are entirely the result of history of X-ray bombardment induced structural states? Surely this analogy overlooks the existence of internal events and processes in the former case. Furthermore, it is at least conceivable that an adult human experiences the consequences of his behaviour, processes this information, and then alters his internal states (wants and beliefs) himself and as he sees fit. On this account it is inconceivable that there could be a law accounting for direct structural changes in terms of act-consequences. Not only, therefore, are internal states not accounted for by such a law, but, because such deliberative processes mediate between internal states and act-token occurrences as well, operant theory cannot begin to explain these events.

Operant theory may of course be relevant to the explanation of act-type acquisition and act-token occurrence in the case of animals other than man, since because of the (apparent) absence of internal processes in the former, their case is analogous to that of glass. Since what is at

issue is the possibility of extending operant theory to account for human behaviour, as well as animal behaviour, however, the question is begged by assimilating human behaviour to the behaviour of inanimate substances such as glass. Thus if the operant conditioning process is totally irrelevant to an account of human behaviour there is hardly any force to the replaceability thesis I have been suggesting. The significance of these objections cannot be overestimated. Nonetheless, I am confident that the causal relevance of internal deliberative processes can be admitted while yet maintaining the fundamental foundational relevance of operant theory. The cash for this claim cannot be provided, unfortunately, until the structural similarities and dissimilarities between action theory and operant theory have been fully displayed. Once this task has been completed, I can suggest replies to the above objections in the context of sketching the implications of operant theory for an evolutionary epistemology.

Before turning to a detailed comparison of central action theoretic and operant concepts it is necessary to summarize briefly the foregoing discussion. The main point of attempting to sort out in detail the confusions surrounding Skinner's attack on explanation in terms of mental entities was to show that, regardless of the ultimate relevance of operant theory to the explanation of human action, the

theory has already been leaving its narrowly empiricist, mechanistic, and behaviourist origins behind. In particular, the general correspondence principle explains how it is consistent to deny the existence and causal relevance of the entities of one conceptual framework while admitting that some such account will be necessary. That is, the GCP rationalizes the rejection of a particular conceptual framework or theory on non-positivist grounds. It also instructs us to expect that a successor theory will not only develop in such a way as to come into a general correspondence with its eventual predecessor, in spite of numerous apparent inconsistencies, but also that it will contain a core of insight that goes beyond its predecessor in a manner that may be very difficult to discern in the absence of historical hindsight.

The foregoing discussion has been an attempt to develop these two themes in light of the GCP. Thus there is a place for structural states in operant theory and Skinner's polemic to the contrary is not totally misguided. Secondly, there may be a core of insight to operant theory consisting of an interpretation which shows the theory to be primarily concerned with act-type generation so that again Skinner's polemic about functional laws and the importance of internal states may be seen as dimly pointing to a valuable theory change.

B. Thus far my comparison of operant and action theory has concentrated on relatively general issues pertaining to the role of internal structure in operant theory and the manner in which operant theory has gradually left its narrower behaviourist S-R and other metaphysical and epistemological foundations behind. I have argued that operant theory may be able to account for the acquisition of particular internal states such as particular wants and beliefs, but that nonetheless there will have to be theoretical laws covering the way in which any wants, beliefs and other states combine to result in the occurrence of act-tokens and perhaps the generation of new act-types as well.

Statements pertaining to the general manner in which internal structure produces action will, consequently, be among the S* statements that survive the transition to operant theory. It seems clear that such statements will pertain to innate processing structure, so that operant theory, or any learning theory for that matter, could not account for anything other than the acquisition of particular wants, beliefs, attitudes, etc. Such a general correspondence obtains between the two theories. However, it is reasonable to expect there to be a similar correspondence between central concepts and distinctions of the two frameworks. The point of the GCP is that such a

correspondence need not be either completely conservative translation or radical incommensurability. The usual sorts of criticisms of operant theory are to the effect that its concepts are so radically different from any action theoretic concepts that there is no sense in which any of the former could replace any of the latter, at least not in a manner in which the alleged successor concepts could be seen to be more adequate to the domain in question than their alleged predecessors.

As I have pointed out previously, due to the fact that adequate analyses of scientific change are only now beginning to emerge, and because neither operant theory nor action theory yet admit of precise formal representation, one can do no more at present than endeavour to show that the usual criticisms of operant theory are wide of the mark by arguing that its central concepts are much more like those of action theory than is generally believed. Obviously, such imprecise claims can do no more than generate further interest. But even with formalization available, ongoing science itself will have to decide the question of the replaceability of action theory by some, perhaps radically revised, future version of operant theory.

In the present inquiry I hope to suggest how some of the concepts of operant theory, as I have been interpreting it, could replace one current theory of human action, that of Alvin Goldman. To keep the project within manageable

proportions I shall have to restrict myself to only four central action theoretic concepts: act, want, belief, and rule. Since there has been no theorizing within operant research pertaining to the role of internal states I shall use arbitrary symbols ψ and ϕ to stand for the successors of want and belief respectively. This is analogous to the mind-body identity theorist's procedure when talking about the brain state referring terms held to be adequate successors to their sensation referring counterparts. The putative successors of acts and rules will be operants and contingencies of reinforcement. It is an empirical matter whether such terms will actually replace those already in use in action theory.

In more radical theory changes new terms are introduced, such as gravitation for natural place and oxygen for phlogiston. In less radical forms of scientific change there is no terminological revision, only a gradual meaning change over long periods of time. In this case we might expect a historian of science of the future to write a history of the gradually changing meaning of the central concepts of action theory much as Max Jammer has done for the concepts of mass, force, and space. Hence, my use of operant terms must not be regarded as excluding the possibility that the putative theoretical change in question will not involve the adoption of operant terminology.

A demonstration of the similarity of operant and action concepts is in danger of provoking a charge of trivial verbal transition. But it is often the case in scientific change, especially during the transition itself, that the resulting theoretical switch in perspective is at first so subtle that opponents are completely unable to see it. Kuhn has discussed a few such cases. It is often only after the successor theory has further diverged from its predecessor that substantial differences are more readily discernible. Because of these facts of scientific change, the apparent triviality of the conceptual revision here envisaged is not alone sufficient to show that the theoretical change really is nothing more than a purely verbal transformation. Without the advantage of historical hindsight, I can do no more than suggest possible increases in generality and explanatory power.

I shall begin with the concepts act and operant. With respect to these concepts the standard criticism of operant theory, and all psychological behaviourisms, is that the behaviourist's conceptual apparatus is too impoverished to be able to deal with anything but mere physical movement. But since action is clearly more than mere movement, so the argument goes, no behaviourist theory could begin to offer a satisfactory explanation of human action. And, indeed, early behaviourists, Skinner included, did suppose they could offer a complete account of all purposive human action.

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solely in terms of observable physical movement. The general impression of all behaviourisms is that they are equipped to explain movement alone and, specifically, a certain form of movement, namely, reflex movement.

The general restriction of the behaviourist's domain is typically accomplished by some such argument as the following: Such behaviouristic theories as operant theory purport to fall within physical science, but action is clearly not reducible to physical movement. Take the act of signing a contract. In the first place, a wide variety of physical movements could constitute an act of signing a contract. Moreover, even a physical description of the entire set of such exemplifications of the act would leave out reference to the set of unobservable wants, beliefs and rules that both give meaning to the act and explain it.

Setting aside the issue of the individuation of actions, it is clear that some light is shed on the first point above by the distinctions between basic and non-basic action and between act-types and act-tokens. Once the positivistic overtones of operant theory are rejected, there is no reason why similar distinctions cannot be drawn with respect to operants. Basic actions are just physical movements, an example of which would be moving an arm. A basic act-token of the type "raising an arm", would be not only an instance at a time of the type, but also a relatively unique instance, since one can raise his arm in an infinite variety

of ways. So even a basic act-type is not some particular physical movement. Non-basic actions are even further removed from physical movements of specific kinds. A much wider class of physical movements is involved. One can turn on a light by flipping a switch, pressing a button, or pulling a cord. A switch can be flipped using any of one's limbs. Furthermore, a much wider class of wants, beliefs, and rules is involved. An accidental flipping of a switch--which is movement--is not an action due to the absence of the requisite want or intention.

The question is whether similar comments can be made about operant behaviour. First, a terminological point is in order; for ease of comparison I shall alter operant terminology slightly. Skinner's distinction between operants and responses corresponds to Goldman's distinction between act-types and act-tokens respectively. I shall employ the terms "operant-type" and "operant-token" for comparative purposes. Apparently there is only a dim awareness, even among operant researchers, of the type-token distinction in operant theory. In Skinner's words "An operant is a class, of which a response is an instance or a member. The usage is seldom respected."¹²

Clearly, therefore, a type-token distinction can be drawn in operant theory. But can we move further beyond purely physical movement by drawing the basic-non-basic distinction as well? With basic acts in mind compare the

following characterization of acts with a corresponding description of operants. According to Goldman:

To perform an act...is to exemplify a property.
...a particular act...consists in the exemplifying of an act-property by an agent at a particular time.¹³

Employing an example of a pigeon's raising its head, Skinner makes much the same point:

The behaviour called raising the head, regardless of when specific instances occur, is an operant. It can be described, not as an accomplished act, but rather as a set of acts defined by the property of the height to which the head is raised. In this sense an operant is defined by an effect which may be specified in physical terms.¹⁴

Notice the restriction to physical properties. This is satisfactory for basic-acts (operants), but an act such as the signing of a contract cannot be fully specified in physical terms. On the one hand, there is a sense in which operants are not narrowly physical, since anything which has the effect of signing a contract would be countable as an operant--provided other necessary conditions obtained. Thus in the simple lever pressing situation any physical movement which has the effect of closing a micro-switch counts as the operant "pressing a lever". But this too is just basic action. In the case of non-basic action the other necessary conditions required for a movement to count as an action are the set of wants, beliefs, and rules that

are peculiar to the context in which the act occurs. Clearly, therefore, in order to move operant theory beyond the range of basic action, the positivistic restriction to physical properties (at least to publicly observable physical properties) must be rejected. It should be apparent that my strategy will be to provide replacement for those further elements which explain and render non-basic action meaningful.

But this strategy will take up the rest of the present section. Before turning to the issues pertaining to this task, I shall approach the operant/action comparison from another angle. As I mentioned previously, when critics of behaviourist theories have charged them with the inability to deal with anything over and above mere movement, they have often had reflex movement in mind. Accordingly, as Care and Landesman, among numerous others, have pointed out, action "is more than mere physiological change such as sneezes, coughs, twitches, and blinkings..."¹⁵ Since traditional S-R theorists, i.e., Watson, did attempt to assimilate all behaviour to a single paradigm modeled on reflex conditioning, this criticism is well founded. However, I have already suggested the correspondence between the operant/respondent distinction drawn by Skinner and the act/reflex distinction.

No questions are begged by this correspondence contention because the theory is not committed to drawing

the operant/respondent distinction simply by asserting that anything which is an act is an operant and anything which is a reflex is a respondent. On the contrary, the operant/respondent distinction can be drawn on independent grounds, and so it must if my central contention is to avoid collapsing into triviality. Respondents can, roughly, be defined as those movements of an organism which, given normal biological functioning, can be elicited by some characteristic stimulus. No such temporally prior stimulus can elicit operant-tokens at all. Temporally prior or concurrent stimuli can acquire discriminative, as opposed to eliciting, causal control during the conditioning or operant-type generation process simply by being (even accidentally) present on the occasion of the reinforcement of tokens of the type to be strengthened or shaped into existence. In the case of respondents there is at least one unacquired eliciting stimulus for each reflexive response. But whether we are referring to natural unconditioned eliciting stimuli or Pavlovian conditioned stimuli, the fundamental causal event is temporally prior to the response. The general relation is between prior event-types and elicited response-types. On the other hand, the fundamental causal event-type in operant conditioning is the reinforcement or "feedback" which occurs after tokens of the type to be strengthened.

It is confusing that the same terminology is used to a large extent to refer to both types of conditioning. As an illustration of the confusing use of "reinforcement" and of the two types of conditioning, consider the following: In an experimental situation, if food is the reinforcer, then in respondent conditioning, it is presented before the occurrence of any behaviour in order to produce a characteristic response--namely salivation. In operant conditioning, the reinforcer is presented after the occurrence of those tokens among the occurring variations of the type the experimenter wants to strengthen. In the former type of conditioning new stimuli acquire eliciting power, whereas in the latter type new behaviour-types are generated. It is true that in operant conditioning, discriminative stimuli are stimuli which acquire discriminative control, but their gaining of such power is parasitic on the causal role of the consequences of behaviour.

The foregoing comments should suffice to show that the operant/respondent distinction can be drawn without the question begging invocation of properties we antecedently know to be attributable to the action/reflex distinction. Even more interesting however is the fact that the operant/respondent distinction preserves, not only much of the sense of the action/reflex contrast, but even the intuitions of those opposed to all causal theories of human action. R. S. Peters and others have made much of the mistaken

application of the categories pertaining to efficient causation in discussing action. But notice that the eliciting stimuli appropriate to respondents occur regardless of what the organism is doing. Such stimuli are paradigmatic efficient causes, so that it makes sense to say of such behaviour that it simply happens. On the other hand, the occurrence of operant reinforcement is contingent precisely and only on what the organism does. Hence the intuitive sense that what an individual does makes a difference to his future is captured in the conceptualization of an operant. As Peters has correctly argued, even if it makes sense to talk of the causation of human action at all, it nonetheless must be the case that two quite different causal models must be operative in the domain of human behaviour such that the different ways in which reflexes and actions are produced is not obscured.

Of fundamental significance, therefore, is the fact that the operant/respondent distinction preserves this intuition in a fashion that is more perspicuous than want and belief causal models of human action. Theories of human action, such as Goldman's, in which internal events are assigned the role of efficient causes completely obscures the difference between these two forms of causation rightfully indicated by commonsense and emphasized

by such philosophers as Peters. Accordingly, the ability of operant theory to highlight the role of the consequences of behaviour in such a way as to preserve the difference between the causal efficacy of events that simply impinge upon us without our doing, on the one hand, and the causal efficacy of the consequences of what we do, on the other, is a significant indicator, if there are any, that operant theory is a reasonable candidate to succeed action theory.

As it will become apparent when I discuss the evolutionary character of operant theory, this aspect of operant theory is one of the fundamental properties that give it greater explanatory power than mentalistic action theory. This point will serve to provide further vindication for Skinner's attack on the explanatory power of mental entities.

Thus far in my comparison of the structure of operant and action theory I have attempted to establish the following two results: (1) Basic operants are conceptually similar to basic acts; and (2) the operant/respondent distinction corresponds to the act/reflex distinction in an illuminating, non-trivial manner. The pressing question to be answered in what follows is just how operant theory must be enriched in order to account for full blooded purposive human non-basic action. It seems that operant theory is

capable as it stands of accounting for the basic action of animals and their non-basic action to the extent that there is such. Animal research is restricted to relatively immediately reinforcing consequences, whereas in paradigmatically rational human action, long deferred goals (reinforcements) are involved, not to mention non-existent goals. Reasons, wants, beliefs, rules, and many other entities are essential ingredients in the explanation of characteristically (non-basic) human action.

As I intend the analysis of theory change adumbrated earlier to show, it will not do to simply graft the concept of reinforcement onto action theory as it stands. The GCP can, and should be able to, guide the construction of a theory informed by operant concepts which has a place in it for a qualified version of action theory but which could succeed the latter. I have supposed that the preferred statements of action theory that will constitute this qualified version S^* that will be preserved pertain to the causal role of such internal states as wants and beliefs. Thus, I must attempt to find conceptual replacements for wants and beliefs. In the semantics of action theory as Goldman develops it the concepts want and belief are employed in the analysis of intentional action.

Now I have argued that operants correspond to actions and that there must be replacements for wants and beliefs. Accordingly, in the semantics of the envisaged operant

theory it must be the case that the successor concepts to want and belief must be employable in the analysis of "intentional" operant behaviour. For the moment it may be useful to mention as a reminder that Goldman is primarily interested in explaining act-token occurrences as opposed to the generation of act-types. Therefore, it is the aspect of operant theory that pertains to the explanation of operant-token occurrence that is here under consideration. The question to be addressed is: How is this aspect of action theory to be qualified by operant theory?

Specifically, I shall attempt to show how it might be possible to analyze intentional operant behaviour without employing the concept of a want. It is imperative to realize that such an analysis would not preclude the causal role of such states vis-a-vis operant-token occurrences. As a label for such states as are referred to by the term "want" I shall employ the symbol " ψ ". In the analysis of intentional operant behaviour it will emerge that the concept ψ is semantically informed by the concept of a reinforcer. The analysis will turn on the possibility of identifying reinforcers with the objects of wants and ψ 's with wants.

Recall the contingent identity I alluded to earlier between having an ability and having an act-type. I identified having an acquired and having an unacquired

ability with having an acquired and having an unacquired act-type respectively. Now consider another, perhaps more contentious, contingent identity:

(3) x is a reinforcer for S at t iff x is the object of a want of S 's at t .

Note, again, that I am not identifying wants with reinforcers. Reinforcers are thus the objects of wants; agents want what they believe are reinforcers for them. Another point of preliminary clarification is that, as in Goldman's theory, we can allow that both acts and consequences of acts can be reinforcers and objects of wants.

The sense in which (3) expresses a contingent identity is quite like Richard Rorty's identification of sensations and brain processes. Thus in such cases of theory change as that involving the replacement of the phlogiston theory, phlogiston and oxygen are contingently identical in the "disappearance" sense: oxygen is what we used to call phlogiston. But the concept of oxygen is theoretically richer than the concept of phlogiston. The same holds for brain process terms over sensation terms. Similarly want and reinforcer are what we used to call want and object of want respectively. The latter concepts are approximations to the former and hence "identical" only in this sense.

Still further clarificatory comments are in order. The above type of theoretical "identity" or better

correspondence has nothing to do with the definition of the terms whose referents are said to be identical. That is, (3) is not a definition of either wants, the objects of wants, or reinforcers. Like "force" in Newton's physics, I believe that "reinforcer" must be understood in terms of effects on behaviour. Just as physical force is something which can produce an acceleration or deceleration in a body moving at a constant velocity, so a reinforcer is something which can bring about a change in rate of responding. Nothing like a logical behaviourist's analysis of psychological concepts in behavioural terms is here being proposed. I am considering, rather, the conceptual manoeuvres involved in a potential theoretical shift. It must also be stressed that the above identity has nothing to do with theories of why events are reinforcing. I am not saying that S finds x reinforcing because x is something S wants. For, if the identity holds, then comments analogous to those I made with respect to abilities and act-types are appropriate. Recall my claim that an agent's acquiring an ability is the same event as his acquiring an act-type, so that both are to be explained by reference to the same reinforcement history.

Similarly: S wants x at t and x is a reinforcer for S at t both because of a particular history of reinforcement. I shall sketch an example to illustrate this point momentar-

ily, but first it is important to mention that I am here referring only to the acquisition of particular wants.

The fact that reinforcement history might serve to explain the acquisition of particular wants does not preclude the existence of a structural mechanism which would make such an acquisition process possible. I shall consider this issue later raising, in particular, the question whether such a mechanism can be said plausibly to order wants and reinforcers in such a way that behaviour is governed by events envisaged as maximally reinforcing. For the present it must be ascertained whether the concept of a want with its semantic link to the concept of the objects of wants can be replaced by the concept with its semantic link to the concept of reinforcement.

In order to substantiate this latter possibility I shall sketch an example designed to show the concept of reinforcement to have more explanatory power than the concept of a want. The general point can first be made as follows: Because of the intentional character of wanting, x cannot become the object of a want without first being wanted. But an unintended event consequent on an act-token can reinforce the corresponding type so that such a consequence or reinforcer can then become the object of a want. For example, objects such as money become particular wanted objects by virtue of being associated with other, especially primary reinforcers. Thus if no events reinforced an

agent's behaviour, he would acquire no wants. That is, the explanation of objects such as money becoming the objects of wants is that they have become conditioned reinforcers. The fact that it is unnecessary to separately condition every individual so as to result in such objects as money becoming conditioned reinforcers obscures the foundational significance of the reinforcement process. I cannot fully account for the implications of our ability to circumvent the actual conditioning process until I come to discuss evolutionary epistemology. That is, it is clearly possible to acquire some wants without first being actually reinforced by the to-be-wanted object. For even if a want was acquired--say in a dream--it would still be true that the wanted object could be used as a reinforcer to control or shape the agent's behaviour in situations where other wants were not overriding or conflicting. Any want an agent has to a certain degree will be such that the wanted object will have a corresponding degree of reinforcing power. Accordingly, intentional action can be characterized or analyzed at least in part by the notion of wanting an object, it should be the case that one can alternately analyze intentional action as action controllable by reinforcement.

If, for instance, a person wanted a certain sum of money, then the sum of money could serve as a reinforcer such that any action leading to it would be more likely to occur if the person were in a similar state of affairs

again. Surely, therefore, the concept of a reinforcer could be used in place of the concept of a want in the analysis of intentional action (operant behaviour). Moreover, my replaceability thesis is enhanced by the fact that operant theory is able to at least partially explain how it is that many, if not all, wanted objects become objects of wants. It also explains how many things can continue as objects of wants due to the continuance of their leading to other reinforcers. I shall argue more fully for the greater explanatory power of the concept of reinforcement and thereby of operant theory later. For now I am simply trying to show that there are no in principle conceptual bars to such a possibility.

In order to further clarify this claim it may be useful to see how it fits in with Goldman's complete analysis of intentional action which is as follows:

Act-token A is intentional if and only if

(a) there is an act (-type) A' such that the agent S wanted to do (exemplify) A', and

(b) either S believed that his doing A would generate his doing A' or S believed that his doing A would be on the same level as his doing A', and

(c) this want and this belief caused S's doing A.¹⁶

So far I have discussed wants and not beliefs. In what follows I shall concentrate on Goldman's condition (c) above and simply assume that if I can make my point with

regard to (c) then much the same can be said of (a) and (b). But before turning to a discussion of belief, a few further clarificatory comments on wanting are in order. Obviously it is possible to want objects that have never existed and will never exist and actions caused by such wants are nonetheless intentional. This fact seemingly clashes head on with the operant theorist's notion of a reinforcer as some manipulable, observable object. But surely this point does not in itself stand in the way of a revision of what it means for an act to be intentional.

If the term " Ψ " represents the state that future neuro-physiology will refer to as that which we now refer to by the term "want", then my analysis of intentional operant behaviour would employ the concept Ψ . This concept would be one which is semantically bound up with the concept of a reinforcer just as our current concept of a want is semantically bound up with the concept of an object of a want. Hence, the fact that a particular reinforcer can for a particular agent be non-existent, no more undermines this operant analysis of intentional action than does the absence of the objects of want undermine Goldman's analysis.

Moreover, at the non-semantic level of the actual causation of behaviour it again is no matter that some reinforcers are non-existent, for the point is that in such cases it will be Ψ 's that cause operant-tokens rather than wants causing act-tokens. The wanting

terminology is only an approximation to the operant semantics. Thus, my claim is not that reinforcing events can alone cause operant-token occurrences independent of internal states such as those we call wants. On the contrary, " ψ " will refer to such states just as "want" now does.

I shall assume that the foregoing discussion is sufficiently clear for the present and turn to the concept of belief. With respect to the role of this concept I intend to make precisely the same contention as that just sketched for the concept of wanting. In this case, however, instead of reinforcement, I shall employ the concept of a discriminative stimulus. The term "discriminative stimulus" is one of the three terms that make up the three term-concept Skinner calls "contingency of reinforcement", the other two terms being "operant" and "reinforcement". Discriminative stimuli enter the causal picture by virtue of being present (normally necessarily-- but sometimes accidentally thereby leading to superstition) on the occasion upon which operant-tokens are reinforced. By virtue of their presence in such situations, they acquire stimulus control over the operant-type, tokens of which were reinforced in their presence. The recurrence of discriminative stimuli raise the probability of the occurrence of an operant-token to whatever level the previous reinforcement induced the corresponding operant-

type. In ordinary language, if an agent is "rewarded" for doing A in the presence of some object, then the agent acquires a belief to the effect that such a reward can (generally) be obtained by doing A in the presence of that object. More precisely:

(4) y is a discriminative stimulus for S at t for act-type A iff S believes at t that the object of a want (x) will follow a token of A in the presence of y at t.

Here again I am referring to an "identity" in the contingent, theoretical replacement sense. Although wants and beliefs are causally intertwined with respect to action, it would be possible to state (4) without reference to wants in order to account for beliefs totally unrelated to behaviour. But since I am primarily interested in the explanation of behaviour, (4) will suffice.

Let " ϕ " refer to the internal state referred to by "belief"; " ϕ " is a term, like " ψ ", that is a part of the structure of future neurophysiology. The claim made by (4) is not that all beliefs about the relation between situation and reinforcement are caused by an operant conditioning process. Some beliefs may be the result of generalization, dreaming, or other sources. The point is that if an agent has a belief that he can obtain some object of some want in the presence of y by doing some specific action, then y can serve as a discriminative

stimulus. And if, conversely, an agent performs an action at the onset of y and not otherwise, then it can be reasonably inferred that he believes that some action is appropriate in the presence of y . Thus, such action oriented beliefs involve an internal state and an object or state of affairs that is related (perhaps causally) to a wanted object. Accordingly ϕ is a concept involving an internal state and the concept of a discriminative stimulus.

Strictly speaking it is " ψ " and " ϕ " which replace "want" and "belief" in the analysis of intentional operant behaviour, rather than "reinforcer" and "discriminative stimulus". However, ψ and ϕ are concepts which derive part of their semantic content from the concepts reinforcer and discriminative stimulus. If the foregoing is on the right track, therefore, operant theory, in the somewhat futuristic form here envisaged, can in principle provide an analysis of intentional operant behaviour that seems sufficient to replace the want and belief action-theoretic framework.

It is imperative to emphasize that it is a completely separate issue to what extent the actual physical occurrence of reinforcers and discriminative stimuli is required in the causation of human, as opposed to animal, behaviour. As I am interpreting operant theory, ψ and ϕ can combine to cause operant-token occurrences much as wants and beliefs are presently conceived to cause act-token occurrences even in situations where no relevant discrimi-

native stimulus is present and no relevant reinforcement has ever occurred.

The foregoing discussion is, I believe, the most reasonable way to develop Skinner's vague remark that an individual behaves as he does at a given time because of his current structure, while also retaining the fundamental insights of operant theory. In general the theoretical revision I am proposing is in accordance with the GCP and common sense. As the GCP demands, there is a very high likelihood that some such internal states as wants and beliefs will play some such causal role as they are said to play in Goldman's theory. Operant theory must develop in such a way as to postulate theoretical states corresponding to wants and beliefs part of the semantic content of which derives from the notions of reinforcement and discriminative stimulus. The actual causal role of reinforcement contingencies is an empirical matter. The possibility that all such internal states (all particular ψ 's and ϕ 's) arise solely through the operant conditioning process and generalization will be considered later. The burden of the present argument is simply that, however restricted the operant conditioning process may turn out to be, it could still be in principle possible to replace current action theory with a suitably enriched operant conceptual framework.

Thus far, three contentions have been argued for:

(1) there are close similarities between basic acts and basic operants; (2) the distinction between respondent and operant behaviour corresponds to the distinction between action and reflex movement; and (3) the analysis of intentional action proposed by Goldman can, in principle, be replaced by an operant analysis. The last contention takes us one step in the direction of moving operant theory beyond the level of basic action. Once the concept of a rule, rule governed behaviour, and rule internalization, is examined, there should be little difficulty in seeing the adequacy of operant theory to account for non-basic action. But before I turn to this task much more needs saying about the causal role of wants and beliefs, and thereby of ψ 's and ϕ 's in operant theory.

So far I have discussed such internal states as though they were related to action in a manner precisely analogous to the manner in which long term dispositional states of such substances as glass are related to breaking. I now want to discuss a class of events that plays a central causal role in Goldman's theory. I shall argue that a slight alteration of the role of these events shows that discriminative stimuli play a much greater causal role vis-a-vis act-token occurrence than is even remotely apparent on Goldman's account. A second result will be

that the greater generality of operant theory will emerge by virtue of its ability to account for both habitual and non-habitual intentional action. This result is significant because Goldman admits that his analysis is not ideally suited for explaining habitual action. The class of events I intend to discuss is occurrent as opposed to standing wants and beliefs.

In Goldman's theory standing wants and beliefs are relatively long term dispositional states analogous to the molecular states of glass. Thus far I have argued that such states could come into existence as a result of the operant conditioning process. I have claimed that it at least makes sense to claim that there could be a functional law of reinforcement that would account for act-type acquisition as well as internal states corresponding to abilities, wants, and beliefs. Now it begins to emerge that episodic entities must be added to operant theory if it is to conform to the predictions of the GCP. Accordingly, in line with the GCP, it must at this stage be shown to be conceivable that events could play a causal role in operant theory without reducing the latter to an entirely minor role. Thus, the role assigned to such events by Goldman must at least be given an operant qualification. I must now provide just such an operant interpretation of internal events while stressing the greater generality and explanatory power of operant theory so as to avoid complete

triviality. As far as generality is concerned, the form of operant theory envisaged herein should be such as to have two special cases, one sufficient for animal behaviour and the other, a qualified version of Goldman's theory, sufficient for human behaviour.

I shall now discuss in detail the contrast between occurrent and standing wants, assuming as Goldman does, that analogous comments are possible for beliefs. But I shall have something to say about beliefs as well. I shall argue that the interpretation of operant theory I am envisaging will need to draw an analogous distinction between standing and occurrent ψ 's and ϕ 's, but that they will be assigned a slightly different role from the one Goldman assigns to occurrent wants and beliefs. The divergence is necessary largely to account for habitual intentional action.

I mentioned above that standing wants, for Goldman, are analogous to the dispositional states of glass. This is not quite correct as the latter give rise to "overt behaviour", whereas a standing want for Goldman,

...is a disposition or propensity to have an occurrent want, a disposition that lasts with the agent for a reasonable length of time.¹⁷

The state of glass and standing wants both give rise to events, but the latter states issue in private episodic wants as short term events, whereas the former yield

observable behaviour. Thus, on Goldman's view "standing wants and beliefs do not by themselves cause acts."¹⁸ They give rise to occurrent wants and beliefs which in turn cause observable action. Accordingly, an "occurrent want is a mental event or mental process; it is a 'going on' or 'happening' in consciousness."¹⁹ Although Goldman does not explicitly say so, it appears that occurrent wants and beliefs are not just events as opposed to states, they are conscious events. The closest Goldman comes to explicitly drawing this distinction is where he characterizes occurrent beliefs by saying that an agent had an occurrent belief at a particular time in the past "when he consciously affirmed or assented to..."²⁰

What I shall argue is that there must either be unconscious occurrent wants and beliefs (ψ 's and ϕ 's) or that, in a sense to be specified, standing wants and beliefs can directly cause some action (operant behaviour). This is not to deny that some action is also caused by the activation of standing wants and beliefs into their consciously occurrent counterparts.

Indeed, Goldman's conclusion alluded to above that standing dispositional states cannot lead directly to overt behaviour is exceedingly quick. Two cases are cited which support his theory and the conclusion is drawn. I shall briefly summarize one of his examples and then construct a counterexample. Goldman has us imagine his

going to the supermarket to buy a number of items of which he has made a mental list. He thus has a standing want to buy each item on his list, one of which is cheese. But when in a position to buy cheese he fails to because "the desire to buy cheese did not become occurrent or activated at this time."²¹ Does this case support the view that all actions are caused by such occurrent mental events?

Consider the following modification of Goldman's example. Suppose Goldman had a standing want to buy cheese at t, did not have the corresponding occurrent want, but bought cheese anyway. On the way home he is thinking through his list in order to be certain he bought everything he wanted and it suddenly hits him that he cannot recall buying cheese. He is not now (and was not at the time) aware of having bought cheese. He glances into his grocery bag and sees that he had bought cheese after all. Now, obviously, everyone has had this sort of experience at one time or other; we do such things unconsciously all the time. But Goldman's theory cannot explain actions of this sort.

Accordingly, we begin to wonder why Goldman was led to such an apparently obviously false theory. Two answers suggest themselves. One is that in rightly opposing the Rylean view that no episodic events cause behaviour, Goldman seems to deem it necessary to go to the other extreme whereby all behaviour is so caused. Surely such

a reason is inconclusive. Secondly, there may be the following general consideration lurking behind Goldman's account: There is the intuitive view pertaining to the causation of events that no object or state or set of states could alone cause the occurrence of an event. Included in the total set of causal conditions there must be at least one event. For instance, a billiard ball cannot cause another billiard ball to move, but the billiard ball (an object) can, in striking (an event) another billiard ball, cause it to move. To take one more example, if the set of states making up the structure of a bridge is such that the bridge is on the breaking point, the states alone will not cause the bridge to break. There must be at least one event such as a single change of state perhaps resulting from excess weight on the bridge.

Similarly, if standing wants and beliefs are structural states, Goldman may have reasoned, there must be some event that causes act-token occurrences. If a theorist wishes to ~~stay within the realm of internal goings-on in his account~~ of action, the obvious candidates for causal events are mental events. But even if this intuition is correct, my counterexample shows no more than that some occurrent wants are unconscious events. Although it is surely possible that there are such unconscious events, I shall argue that at least some action could plausibly be said to be the causal result of a combination of standing states (ψ 's and

ϕ 's) and events such as the onset of discriminative stimuli.

Consider again the example of the buying of cheese. Goldman had a standing want to buy cheese at t , and although he was distracted or thinking deeply about something else at t in the presence of cheese, nevertheless, the stimulus-onset, the appearance of cheese, operating directly in conjunction with internal standing states caused him to buy cheese. How else are we to explain our actions in which we only consciously realize we have done them after we have done them? Surely, therefore, there is a large class of unconscious and habitual, but intentional, actions which operant theory is ideally suited to handle.

I shall now argue that even in the cases Goldman discusses there is a role played by discriminative stimuli.

The fundamental problem with Goldman's analysis of occurrent wants, as I see it, is an equivocation on the concept of an event. Once this confusion is eradicated it will be apparent that occurrent wants can be, on occasion, regarded as occurrent states rather than events, in which case some event is still required to cause the occurrence of an act-token. I shall argue that the necessary event in such cases is the onset of a discriminative stimulus. The tension in Goldman's use of the concept of an event obtains between regarding an event, on the one hand, as an instantaneous change of state, and on the other, as a brief

but continually steady state. As Kim and Alston have pointed out, we do commonly refer to short term steady states as events if they occur against the background of a much longer unchanging state of affairs. Thus, for instance, a light that comes on, lasts a few seconds, or even a few minutes, might be regarded as the only event of the whole night by a lonely sentry.

It seems quite clear that Goldman does not distinguish between these two senses of event. In the case of the light we would include the entire, albeit short, duration as the event and not just its onset and termination. Now presumably it is events in the first sense, events as instantaneous state changes, that are paradigmatic causes and effects. This seems correct for the following reason: If the event which is the onset of a short term steady state (event in the extended sense) does not cause anything, then if nothing else changes, the continuance of the steady state will not cause anything either. Hence if some of Goldman's occurrent events are only events in the extended sense, then they will not alone cause any act-token occurrences.

What grounds are there for the suspicion that Goldman has confused these two senses of event? Consider the very first example Goldman employs in order to illustrate the concept of an occurrent want:

John is concentrating on finishing the lawn by six o'clock. He is giving all his attention to mowing it as quickly as possible, with the thought of getting it done by six. During this period, whether it be a few seconds or a whole minute, John has the occurrent want to finish mowing the lawn by six o'clock. The thought of finishing the lawn by six occurs to him, occupies his attention, fills his consciousness. It is a datable event or process.²²

Clearly, with the reference to "a few seconds or a whole minute", Goldman thinks of occurrent wants as events in the extended sense. But when he says "the thought of finishing occurs to John", it is the sudden onset of a state or change of state that is suggested. The sudden thought is actually the onset of the occurrent want qua steady state. Thus when Goldman mentions the continuance of the thought in John's consciousness, an event in the extended sense is involved.

The obvious and pressing question is the one I alluded to above: Which type of event is causally related to action? Both? In the same way? Surely not. It is important to keep in mind the point I suggested above when discussing the strategy of my present argument: If an agent is in standard conditions such that he would do A if a want of the requisite sort were present, it would be the case that if the onset of the want were not sufficient to trigger the action, then, unless other conditions change (events occur), the continuance of the want would not be sufficient to trigger the action either.

The significance of this point can be illustrated nicely in the context of Goldman's example of buying cheese. Suppose that, unlike the onset of the want to finish the lawn by six, the onset of the want to buy cheese occurs several seconds prior to Goldman's being in the presence of cheese. Now in a case such as this it is clear that the agent can be characterized as being in, or having a standing want and an occurrent want-state. There is no want qua instantaneous state-change occurring just as the agent sees cheese and reaches for it. On the contrary, it is the onset of the sight of, or presence of, cheese that is the requisite type of event we need in order to properly fill out the causal picture. But, interestingly, such an event has the look of a discriminative stimulus onset! This important fact is missed by action theorists, like Goldman, who construct their examples with their agents already in the decision making situation. It is thus only natural to look inside the agent for events to label as the causes of action.

Accordingly, Goldman is confusing two types of cases. If one is already in the presence of cheese and looking at it; then, clearly, the onset of a want is just the sort of requisite causal event that will trigger the action. But if one is looking for cheese already in the occurrent want-state (event in the broad sense), then the causal event vis-a-vis the act of buying cheese is the onset of

the discriminative stimulus, cheese. In the latter case the occurrent want-state may still be regarded as a necessary condition and in the former the discriminative stimulus qua object, as opposed to event, would be a necessary condition. In a third type of case, discussed earlier, where an agent can habitually or unconsciously do things the standing want-state to do A at t would be the only necessary condition; no occurrent want-state would be necessary. In this case, again, discriminative stimulus onset would be sufficient. In a fourth type of case imagined discriminative stimuli would be causally relevant either as necessary or as sufficient conditions. In this type of case the physical presence of discriminative stimuli does not play a role. This is not necessarily a difficulty for operant theory since such actions are still caused by ψ 's and ϕ 's in which the concept of a discriminative stimulus is important.

The foregoing discussion carries operant theory a long way toward coming into a general correspondence with action theory. Just as the GCP predicts, operant theory has given us a way of qualifying action theory; it explains the way in which action theory has obscured the role of impinging environmental stimuli which have acquired causal control by virtue of having act-tokens reinforced in their presence. It is also in line with the idea that it is not sufficient for an action to want to buy something like cheese unless you

also believe that the object in front of you really is cheese. And this is to say that the object must be functioning as an appropriate discriminative stimulus, or at least believed to be: the agent must be in the appropriate state.

With the exception of these important qualifications of Goldman's theory as it pertains to act-token occurrences, his theory survives the transition to operant theory almost intact. I say "almost" because there is no predicting, at this stage, what further qualifications might turn out to be necessary, or whether the meaning change would be substantial enough to warrant the suggested terminological alterations.

In this broadened operant theory we see the importance of at least the concept of reinforcement relative to that of wanting. The role of real discriminative stimuli as well as the concept of such is now apparent. Moreover, in the successor theory we have a unified account of both habitual or unconscious action as well as conscious action. And lastly, although I have not yet fully argued this aspect, we have an account of the acquisition of internal states and act-types. By thus coming into a general correspondence with action theory, operant theory is seen to be more general; it can account for act-token occurrences not accounted for by Goldman's theory, and it can supplement the account of many other types of act-token

occurrences. It, therefore, has at least as much if not more explanatory power. I shall discuss more fully its account of act-type acquisition later.

But first it is imperative to elaborate on the role of two further central action theoretic and operant concepts with respect to the explanation of act-token occurrences. I have in mind the possibility alluded to by Skinner to the effect that rules are abstracted or extracted out of contingencies of reinforcement. Rule-following action is commonly regarded as paradigmatic of the sort of behaviour that falls totally outside the scope of any behaviourist theory. I shall argue that just as "operant" can replace "act", just as " ψ state" can replace "want", and just as " ϕ state" can replace "belief", so "contingency of reinforcement" can, in principle, replace "rule". If this contention, or minimally one to the effect that an understanding of contingencies of reinforcement could force an alteration in the meaning of "rule", is sustainable, then the plausibility of replacing action theory by operant theory would be greatly enhanced. For we are accustomed to regarding very much of our purposive behaviour as stemming from sets of internalized rules. Hence if such sets of rules are really just sets of contingencies of reinforcement, and if operant theory can even explain their very internalization, then the majority of the conceptual bars to such a

theory change as is here envisaged would be eliminated. One of many interesting questions is this: Can the common-sense distinction between being guided by implicit rules and consciously following rules be drawn within operant theory?

Skinner certainly thinks that operant theory is sufficiently adequate to meet such demands. His rather vague suggestion of how verbal communities formulate rules is intuitively plausible at least. He simply points to the reinforcing consequences of the behaviour of abstracting rules from contingencies of reinforcement. What is necessary in order to evaluate this contention is a careful comparison of the structure of rules and contingencies of reinforcement. A good place to begin as any is with a look at Skinner's own statement of this contention:

It is all very well to say that we extract rules from contingencies of reinforcement, either when we have been exposed to them or have had the chance to study the systems which arrange them, and that we gain by doing so because we and others can then follow the rules rather than submit to the possibly tedious process of having behaviour shaped by the contingencies. But "extracting a rule" is complex behaviour, and the natural reinforcement may be deferred. Why and how does the behaviour arise?²³

Naturally, Skinner answers this question in terms of the reinforcement of successive approximations to the full-blown rule extracted behaviour. But of primary interest in the above passage is its conciseness as a statement

of Skinner's view on this matter. Rules are not only abbreviated and abstracted contingencies of reinforcement, the behaviour of formulating such rules is itself explainable in operant terms. The contention has an intuitive air of plausibility to it. If it is admitted that contingencies of reinforcement control behaviour in the absence of rules, it is easy to envisage the reinforcing power of the convenience accruing to explicit recognition and extraction of the rule. In order to sustain this highly interesting claim it is essential to compare the structure of reinforcement contingencies with that of rules. Since rules are commonly contrasted with empirical generalizations, it will lend weight to my case if I can show that reinforcement contingencies have more properties in common with rules than they do with empirical generalizations.

A convenient account of the differences between rules and empirical generalizations is contained in a recent work entitled Rule-Governed Linguistic Behaviour by Raymond D. Gumb. The purpose of this account of rule-governed behaviour is to delineate what Gumb refers to as "five marks of social rules" that distinguish rules from empirical generalizations. My aim, therefore, will be to ascertain the extent to which Gumb's "five marks" can be attributed to contingencies of reinforcement. For his first two marks, Gumb follows Peter Winch:

The first mark of social rules is that they must be teachable and the second mark is that it must be possible to misapply a rule or make a mistake.²⁴

This mark quite clearly fails to apply to empirical generalizations; the objects they range over do not have to learn to behave in accordance with them. Thus, as Gumb correctly argues,

Behaviour conforming to a social rule...is learned. On the other hand, objects do not learn to conform to empirical laws, because they MUST (a physical modality) conform to them.²⁵

Now it is precisely this sense of rule-conforming behaviour that seems to be captured by the notion of behaviour under the control of contingencies of reinforcement. One must learn to behave in accordance with particular contingencies of reinforcement. That is, if a certain complex behaviour is to be brought under the control of a certain subtle reinforcement contingency, individuals must be "taught" to so behave by someone using other contingencies and gradually shifting them in order to generate successive approximations to the final desired form. Obviously, because of the occurrence of "wrong" variations there will be "mistakes" made. The reason it is inappropriate to label such variations mistakes is that, far from being undesirable side-effects of the learning process, they are

necessary ingredients in all learning.

Contingencies of reinforcement are thus unlike empirical generalizations in that individuals must learn to behave in accordance with them and mistakes can be made. Moreover, empirical generalizations are descriptive, whereas contingencies of reinforcement have a more prescriptive overtone. We can describe what contingencies are in fact controlling an individual's behaviour, but the contingencies themselves are not descriptions. Nonetheless, there is some degree of similarity between reinforcement contingencies and empirical laws. Although particular contingencies are not lawlike, there is still some yet to be precisely formulated law, some version of the law of effect, stating the general relationship between the three terms embodied in contingencies of reinforcement, i.e. behaviour, discriminative stimulus, and reinforcement. But this is no problem; anyone subscribing to a causal theory of human action will have to hold that there must be some law governing the way in which we learn to behave in accordance with rules. Operant theorists will agree: It will be something like the law of effect.

Contingencies of reinforcement thus seem to satisfy Gumb's first two marks of social rules. The question to be answered now is whether there are further structural similarities obtaining between rules and reinforcement contingencies. Rules specify what behaviour is required

or appropriate in specific circumstances if some consequence is to be forthcoming. Rules are often, if not always, taught and maintained through the use of reinforcement and/or punishment. So far rules appear to be contingencies of reinforcement. However there is an apparent difference between rules and contingencies of reinforcement to the effect that rules seem to specify only two of the three terms constituting reinforcement contingencies: situation or context and behaviour. In other words it would appear that rules are connected with some specific form of reinforcement or punishment, only when an agent is being taught the rule or only when the rule is being enforced. Accordingly, there appears to be an essential connection between the three terms of reinforcement contingencies, whereas rules involve only two terms, with reinforcement and punishment serving merely as an instrument used to teach or enforce rules.

But, far from being an objection, I think this precisely captures the sense of extracting rules from contingencies of reinforcement that Skinner is driving at. In teaching rules to children, or in exploring the physical or social world ourselves, there are specific consequences contingent upon tokens of act-types in specific tokens of context-types. For example, social approval or disapproval may take highly specific forms in the shaping of the behaviour of children. But due to the fact that everyone

reacts differently to the behaviour of a given individual, such specific reinforcements and punishments become thoroughly generalized by the adult stage of an individual's life. Thus the specific connection to some particular consequence is gradually lost by the rule as children grow up. On the other hand, specific forms of consequence can certainly recur whenever an individual deviates. In the enforcement of conformity the status of rules as reinforcement contingencies reasserts itself as clearly as it obtains in the context of the first teaching of rules.

~~Satisfaction of the first two marks of social rules~~ lends a great deal of weight to the contention that rules are nothing but abstract verbal formulations of contingencies of reinforcement. Clearly, if the first two marks are attributable to contingencies of reinforcement, the remaining three should also be readily satisfied. The third mark is simply that "they must be reflected in behavioural regularities of the group."²⁶ This mark is not too interesting since the same can be said of empirical generalizations or laws. However, there is less of a tendency for the latter to be violated, whereas in the case of rules, many individuals within a group can deviate from the set of rules that constitute and define the group.

Contingencies of reinforcement are precisely like rules in this respect. They too can be enforced in a

community or group and can thus constitute that group and govern the behaviour of its members. But there can be a large degree of individual deviance, simply because the existence, and perhaps weak enforcement of contingencies of reinforcement, is not alone sufficient to determine the behaviour of each member of the group. For each individual will have a unique history of reinforcement, so that many individuals who have been reinforced for deviant behaviour that is incompatible with also behaving in accordance with the group-recognized contingencies, may rarely behave as the group would prefer.

The fourth mark is that "mature agents cite them in justifying and criticizing behaviour."²⁷ Here we touch on the controversy over the role of reasons and causes in explaining behaviour. Because of the existence of sets of rules to which agents are bound by virtue of membership in a community, it is naturally asserted that such rules must be cited as reasons in the explanation (justification) of an agent's behaviour rather than personal causal factors. This intuitively correct idea can be expressed in operant theory as follows: In justifying behaviour, an agent ought to cite those contingencies of reinforcement that constitute, or are recognized by, the agent's group. It will not do to point to one's unfortunate history of reinforcement if it is the result of contingencies that clash with those upheld by one's group. "Causes" and "reasons" coincide

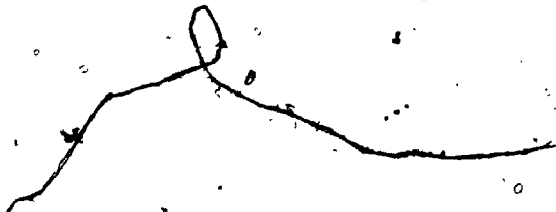
when one's behaviour is causally generated by the contingencies that define one's group and which are upheld by its members.

The sole remaining mark of social rules adumbrated by Gumb is one whereby "it must be possible for agents to conform to them."²⁸ An agent's behaviour corresponds to, or conforms to, a contingency of reinforcement when the requisite behaviour is caused by that contingency and not some other. The fact that, on the occasion where an agent either conforms or does not conform to a particular reinforcement contingency, there are real alternatives is due to the fact that there is no law compelling an agent's history of reinforcement to be of the requisite sort. What alternative the agent chooses will be a function of some history of reinforcement and some prevailing contingency of reinforcement, but the latter may or may not be the contingency to which conformity is expected by others.

One of the great clarifying features of the idea of replacing action theory with operant theory is that much of the mystery surrounding the seemingly unbridgeable gulf between rules and reasons, on the one hand, and causes on the other, dissolves, or so it seems to me. Moreover, many philosophers have found the concept of a rule and the idea of rule-governed behaviour suspicious because of the linguistic appearance of rules. But regarding rules as

reinforcement contingencies allows us to discern much more clearly how rules relate to both behaviour and the physical and social world. The relation between rules and reinforcement contingencies thus deserves a much deeper investigation than has been attempted here or anywhere else. Reinforcement contingencies can govern behaviour (act-token occurrences) without the agent's recognition of the "rule". Viewing rules in such contexts as contingencies of reinforcement seems somehow much simpler and more intellectually satisfying than attributing tacit or implicit knowledge of the rules to the agent.

On the latter view the agent appears to know in some sense what the rule is before he can behave at all. On the other hand, the operant explanation in terms of the gradual shaping of behaviour by subtly shifting contingencies with the agent gradually becoming explicitly aware of the connection between behaviour, situation and consequence seems to make the whole process remarkably transparent. On this view internal structure gradually gains control as awareness emerges. Exposure to contingencies thus alters the behaviour and internal structural states together. Explanation in terms of tacit knowledge utilization seems to render the explanans more mysterious than the facts to be explained.



The foregoing discussion has served to compare rules with reinforcement contingencies largely in order to dispel any suspicion of conceptual absurdity appearing to accrue to the theoretical revision I am sketching. I have also hinted at the potential increase in explanatory power of the suggested theoretical shift. If the argument is plausible, then to the extent that we view ourselves as behaving to a large degree as a result of sets of internalized rules, then we can regard our behaviour as stemming from similar sets of internalized reinforcement contingencies. I shall now comment on such an internalization process in somewhat greater detail in order to shed some light on how operant theory could possibly distinguish between unconscious rule guided behaviour and conscious rule following. After this account is presented I shall attempt to show that the theoretical change here envisaged is not just a verbal transformation by arguing for its greater explanatory power. This will take me into the evolutionary aspect of the theory in which, among other things, I shall compare some of Skinner's remarks on the relation between rules and reinforcement contingencies with comments by Kuhn on a similar relation between rules and paradigms.

But first a few more brief comments on the internalization of contingencies of reinforcement. This notion, along with the elaboration of the role of internal states,

takes us a long way toward making sense of Skinner's vague allusion to the relation between current structure and behaviour. The resulting picture of operant theory is no longer one of organisms, humans especially, behaving solely as a result of currently impinging environmental stimuli. The older behaviourist view which still taints Skinner's writings portrays the agent's act-token occurrences as the result of environmental efficient causes alone. Once internal events and states are shown to play a causal role, there is no reason to disallow internalized rules (contingencies of reinforcement). At least two forms of such internalization can be distinguished.

One such form would result from direct and repeated exposure to contingencies of reinforcement. The contingencies in this type of case are not extracted as rules by the community or by the agent; the agent's internalization process proceeds without his awareness of the "rule". But this form of internalization amounts to no more than the acquisition of an ability (operant-type), a standing want (corresponding to the internalization of reinforcement), and a standing belief (corresponding to the acquisition by a stimulus of discriminative control). Clearly, even animals can internalize "rules" in this sense.

The second form would go beyond the first by the addition of conscious recognition of the relation between

the stimulus situation, the requisite operant-type, and the reinforcement. The gradually extracted rule can then be consciously applied in the sense that it can play a role in deliberation or planning. I see no reason in principle why such gradual extraction to the point of conscious elaboration cannot itself be accounted for by the differential reinforcement of successive approximations.

The upshot of much of the foregoing brief discussion is that the commonsense rule following model of human action survives the theoretical transition with little more than a reinterpretation. But once we see this model as only a special case of a more general theory we lose the temptation to use such a model to account for all, even habitual or unconscious, human action. Accordingly, the more an agent's behaviour approximates the ideal of the consciously rational, rule following agent, the more we enter into what Koertge calls the set of likely-to-be-unrevised preferred statements. These considerations suggest that two approaches to the explanation of behaviour are equally misguided. One approach consists in extending a Skinnerian account of animal behaviour unrevised to account for human behaviour as well. The other consists in taking the model of the ideally rational agent consciously following rules as the correct theory and explaining all other behaviour as though it were mere deviation from the ideal.

Both tendencies stem from a philosophy of science which regards theories as totally static entities which either cover the entire domain they purport to embrace or are refuted in their entirety. A more appropriate view might be to regard both theories, Goldman's and unrevised operant theory, as approximately correct in their respective domains but mergeable into a general theory which yields both sub-theories as special cases. Rather than opposing the two theories, we would be in a position to explore their very real conceptual overlap as I have fragmentarily done here. It would be a mistake, in other words, to restrict the purely Skinnerian sub-theory to animal behaviour and perhaps much of the behaviour of children and habitual but intentional adult behaviour. Nor should the conscious episodic sub-theory be restricted to the adult human domain. There is no such sharp division. Children are capable of following rules more rationally than adults on occasion, and adult humans are capable of emitting act-tokens and acquiring new act-types as unconsciously as other animals on occasion. And even in the most consciously rational situations, the conceptual framework of operant theory is of explanatory relevance. The exact interrelation obtaining between the two theories is largely an empirical matter.

Enough has now been said of rules, rule internalization, and their counterparts in operant theory. Further

support for my claims, along with an exploration of their ramifications, will now follow. I shall relate the foregoing discussion to an analysis of scientific change and to Kuhn's views in particular. The quasi-evolutionary aspect of operant theory should then become more apparent. First, I shall compare Skinner's vague remarks on the relation between rules and reinforcement contingencies with Kuhn's equally imprecise comments on the relation between rules and paradigms.

Accordingly, consider the following characterization of the relation between rules and reinforcement contingencies offered by Skinner:

It is the contingencies, not the rules, which exist before the rules are formulated. Behaviour which is shaped by the contingencies does not show knowledge of the rules. One may speak grammatically under the contingencies maintained by a verbal community without "knowing the rules of grammar" in any other sense, but once these contingencies have been discovered and grammatical rules formulated, one may upon occasion speak grammatically by applying rules.²⁹

A strikingly similar relation appears to obtain between paradigms and rules on Kuhn's account of scientific change: "Rules...derive from paradigms, but paradigms can guide research even in the absence of rules."³⁰ Or again:

The determination of shared paradigms is not, however, the determination of shared rules...

[the]...object is to discover what isolable elements, explicit or implicit, the members of that community may have abstracted from their more global paradigms and deployed as rules in their research.³¹

And finally:

Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research...Indeed, the existence of a paradigm need not even imply that any full set of rules exists.³²

Of course it would be stretching the truth to suggest that what Kuhn has in mind when using the term "paradigm" is precisely what Skinner has in mind when using the term "contingency of reinforcement". Nevertheless, the striking similarities apparent in the above passages are more than merely suggestive. Both paradigms (at least on some uses) and contingencies are agent-environment relations that can guide behaviour in the absence of full awareness of the precise nature of the relations. On both accounts rules are gradually recognized and extracted. This is just one point of similarity with Kuhn's account of scientific change. Forgetting for the moment the question of the adequacy of Kuhn's treatment of the history of science and the question of the existence of sharp divisions between normal and revolutionary science, I shall now allude to a number of further points of similarity. This procedure should clarify the sense in which operant theory is primarily an evolutionary theory of act-type generation

and also show how it is more fundamental than theories of action such as Goldman's. The result will be a reply to the criticism of operant theory earlier mentioned that internal states, events, and processes could account adequately for most if not all act-type generation and act-token occurrence.

According to Kuhn we can think of scientific behaviour under a paradigm as a set of stereotyped act-types gradually expanding as the paradigm is articulated. The only way entirely novel act-types and thereby paradigms can emerge is through the awareness of anomaly. Now anomalies are often the unanticipated consequences of puzzle solving action. They come to be recognized as discoveries. Since such consequences of such actions are the sole source of new paradigms it follows that they are the sole source of all paradigms. Now such unexpected consequences lead to an increase in variation in the investigator's behaviour. On Kuhn's view "nature itself" selects out the correct variation which eventually leads to full awareness and a new set of rules.

Now this is precisely what happens in the operant conditioning process. It is the differential reinforcement of successive approximations to novel act-types. When an act-type becomes relatively stereotyped at a specific stage with only a slight amount of variation among act-tokens, and

when reinforcement is discontinued for that precise type and made contingent upon a slight variation, the result is an immediate widening of the range of variation until one of the tokens in the thus widened range leads to reinforcement. Such shifting contingencies are analogous to violated expectations, i.e. anomalies. But the most important point is that Kuhn recognizes the need for variation if nature itself is to have a chance to differentially reinforce the correct alternative. Thus it need not be the case, as Kuhn believes, that anomalies precede all new paradigms. For much scientific advance may be more as Feyerabend imagines it, namely, with a fair amount of variation existing all of the time.

Feyerabend carries the need for random variation to extremes, however. But both writers recognize the role of variation. This begins to suggest why we can assign a less significant role to internal states and processes in the generation of fundamentally new act-types... For if we emphasize the role of wants, beliefs, strategies, etc., we will tend to interpret unexpected consequences on the basis of preconceived beliefs and expectations. The only way to break out of the circle of beliefs and expectations is to maximize the chance of producing novelty. This requires variation, the more random and relatively incomparable the better. On the biological side, species that cannot adapt to a new environment are species incapable of

variation. Ironically, since variation is thus, according to operant theory, a necessary condition for the emergence of new act-types, its employment is rational, not irrational as Feyerabend seems to conclude. Accordingly, we can want to employ a fairly random trial and error strategy, but we cannot want to acquire the specific act-type that ultimately gets selected out of the quasi-trial and error variations. Direct exposure to contingencies of reinforcement ultimately accounts for all new act-types.

Consider the evolution of human act-types from the emergence of man to the present. At any given time period we can observe the act-types of others and simply want to follow suit. But act-types new to mankind as a whole cannot emerge this way. This suggests a fundamental error in empiricist philosophies and ego-centric outlooks generally. As consciousness emerges in an individual, he first becomes aware of the relation between his own internal states and his behaviour--act-token occurrences and act-type generation. Because he is not aware of the role of consequences of actions vis-a-vis his emerging awareness and act-types, he naively thinks himself fundamentally responsible for them. But the fact that we can generate new act-types ourselves as individuals does not undermine the ultimate role of reinforcement in selecting and developing entirely novel act-types.

It is not that we cannot think of new act-types

ourselves, but rather that all such act-types must be
 "tried out in the world itself" which is thus ultimately
 responsible for those selected. One has to keep in mind
 not only the role of reinforcement in shaping new act-
 types but also its role in maintaining presently existing
 ones. Unless reinforcement is at least intermittent, act-
 types extinguish. Accordingly, it is just to fail to see
 the point to object that a consciously rational agent is
 in complete control of which act-types he is going to allow
 to be selected because he can simply accept the consequences
 of action and decide between them on his own. The
 objection continues by pointing out that act-consequences
 do not directly alter act-types and internal states. But
 this is to overlook the possibility of regarding such
 "rational" behaviour as itself behaviour which, in those
 individual's employing such "strategies", is also the
 result of a prior reinforcement history. Certainly, one
 can strive to circumvent the direct operant conditioning
 process by consciously weighing alternative consequences,
 as much as possible, but this is to overlook the ultimate
 fundamentality of the reinforcement process. We only
 strive to behave in such a manner because it does in fact
 have reinforcing consequences.

Reinforcement is thus causally basic because it, like
 natural selection, is the ultimate selector and sustainer
 of whatever behaviour occurs. Natural selection too

requires systems containing states and internal processes and these are necessary stages for the variations that will lead to new states, processes, and systems.

None of this argument is intended to preclude the foundational role of something like maximization of expected utility, but I hope I have given some sense to the centrality of the reinforcement process. It is with its ultimate role in mind, I believe, that Skinner maintains the explanatory powerlessness of mental entities. The objection is that current structure is not both necessary and sufficient for the emergence of new act-types or species. It is necessary, but so also is reinforcement; and given that there would be no current structure and current act-types without reinforcement, the latter is fundamental.

The foregoing is a very sketchy and speculative account of the implications of operant theory for the development of an evolutionary epistemology. The main point is that the ability of specific individuals at specific times to produce act-types and token occurrences relative to a stable background of beliefs is a truth which is of relatively little interest when the dynamic evolution of human act-types as a whole is considered. Much needs to be said about just how such an epistemology would differ from other epistemologies. Unfortunately this project is beyond the scope of the present study which has included

an allusion to such an epistemology only to give some sense to the alleged fundamentality of the reinforcement process and thereby operant theory in explaining behaviour.

I shall thus leave the suggested epistemological revision and conclude this stage of my argument with a few comments on internal processes such as thinking. Clearly, little needs to be said about such processes over and above that already said about other internal states and events. We have already seen that both Watson and Skinner talk of thinking even if perversely as "covert behavioural processes". As far as the present foundations and structure of operant theory is concerned, the ontological realization of such processes can be left open. The main operant contentions are twofold: (1) the process of thinking can be understood using overt operant behaviour as a model; and (2) learning to think is in some sense parasitic on the learning of overt operant behaviour, including verbal behaviour by the differential reinforcement process. Thus instead of talking of sequences of mental acts occurring in accordance with internalized rules, we simply talk of covert operant chains and internalized contingencies of reinforcement.

As we have seen in citing Skinner's comments on thinking, practical reasoning is regarded as the considering of the reinforcing power and reinforcement likelihood of alternate courses of action. The point is simply that

surely such a process can be conceptualized in operant terms, once it is admitted that the concepts of act and rule can be replaced by the concepts of operant and reinforcement contingency, and once it is allowed that the concepts of want and belief can be supplanted by those of ψ and ϕ respectively.

Consider the following two objections. How is operant theory so fundamental if quite novel act-types and act-tokens can have as their source long and involved thought processes, deliberations, and planning? What about the, possibly large, number of act-tokens which do not require for their occurrence the actual physical presence of discriminative stimuli? Indeed, these facts constitute distinguishing features that mark man off from other animals. In general, what seems to distinguish man from other animals, is the capacity of the former to circumvent to a very large extent the direct operant conditioning process. These are important considerations, but entirely wide of the mark. For it is only possible to avoid operant events by employing act-types that themselves may have resulted from prior direct operant conditioning. Thus the above objections miss the fundamental evolutionary aspect of operant theory. It is at least a conceptual possibility and an empirical plausibility that man today is only able to employ strategies, imagine discriminative stimuli, be "informed" rather than changed by the consequences of his

actions because of the differential reinforcement of more primitive approximations to such complex behaviour. Again, if operant theory is true, we have some light shed on just why we are so misled by our current ability to juxtapose complex covert behaviour between environmental contingencies and overt behaviour. Again, therefore, if operant theory is true, the causal role of such covert deliberative processes vis-a-vis either some act-type generation or some act-token occurrences need not be denied. But this concession hardly undermines the ultimate fundamentality of reinforcement if these very abilities (act-types) can themselves be accounted for by direct operant conditioning which may have occurred sometimes prior to the emergence of human awareness in the full sense. It is thus in admitting the role of such covert processes that the essentially evolutionary character of operant theory becomes most fully apparent.

The major stages of my argument are now complete. Operant theory can, in principle, replace action theory for the following reasons: (1) It is more general by virtue of pertaining to the act-type generation and act-token occurrence of all animals; and (2) It has gradually come into a general correspondence with action theory such that a qualified version of the latter as it pertains to act-token occurrence and some act-type generation has a place within operant theory. Moreover, it has become apparent

that the usual metaphysical contrast between mechanism and teleology simply loses much of its force and interest.

I shall now further clarify the interpretation of operant theory sketched thus far by showing what it implies for foundational issues in contemporary learning theory and by replying to some standard criticisms of the theory. In particular I shall comment on a form of learning alleged to be independent of operant processes called modeling as it has been investigated by Albert Bandura. I shall also discuss the putative role of awareness in learning. And of standard criticisms I shall focus on those of Chomsky.

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V. FOUNDATIONAL ISSUES IN LEARNING THEORY

A. In the first part of this section I shall discuss the so-called modeling process and assess the claims of Bandura to the effect that it constitutes a distinct form of learning, not reducible to operant conditioning. Upon examining Bandura's "social learning theory", as he calls it, one is struck by the following two features: (1) it is presented within an entirely operant conceptual framework; but (2) it seems to diverge markedly from operant theory as one finds it in Skinner's writings. Bandura has no qualms about talking about all sorts of cognitive processes and apparently non-operant functions of reinforcement. But given the picture of scientific change adopted herein, whereby genuine and fundamental theoretical novelty emerges gradually and with difficulty it would be rather baffling if Bandura's apparent divergence from operant theory as I have interpreted it were as great as a first glance would lead us to believe.

Thus I shall attempt to establish the following points: (1) The modeling process is a unique form of learning in the sense that it is a real empirical phenomenon not investigated in any detail before Bandura. Its elaboration therefore constitutes a significant empirical advance over earlier operant theory. (2) Nonetheless, the

degree of divergence between the version of operant theory sketched herein and Bandura's social learning theory is very slight. It will emerge that it is at least plausible that modeling is a complex act-type which is like the processes of circumventing direct operant conditioning alluded to earlier in that it has itself been acquired through direct operant differential reinforcement. However, if it should turn out that even animals are capable of modeling, then such an "act-type" would only be explainable by operant theory in its most all-encompassing form, namely, as the theory of evolution by natural selection itself, or by whatever theory should subsume both operant and evolutionary theory. This result would, nevertheless, show that direct operant conditioning is far from the sole form of learning in complex species of organisms currently existing.

I shall begin my discussion of modeling with a comment on a criticism levelled by Bandura at earlier behaviours such as Skinner's:

A...valid criticism of the extreme behaviouristic position is that, in a vigorous effort to eschew spurious inner causes, it neglected determinants of man's behaviour arising from his cognitive functioning. Man is a thinking organism possessing capabilities that provide him with some power of self-direction. To the extent that traditional behavioural theories could be faulted, it was for providing an incomplete rather than an inaccurate account of human behaviour.¹

This is a fair assessment. Although Skinner has discussed the role of thinking and self-control, it is certainly true that he regards his own investigations as only part of the picture. But more importantly, how are we to understand Bandura's reference to cognitive processes. At first glance he seems to be diverging markedly from Skinnerian operant theory. And, indeed, perhaps the processes he alludes to are fundamentally unique in some sense. No conceptual considerations such as are found in the present inquiry could show otherwise. However, there is nothing conceptually absurd in the suggestion that such cognitive processes and abilities of self-direction themselves can be accounted for in more narrowly operant terms. On the contrary, if operant conditioning is as much like natural selection as I have been suggesting, then it is entirely plausible that some such process is causally responsible for the abilities Bandura mentions.

Could not much the same be true of the modeling process and of observational learning generally? I shall now discuss these processes and argue for the plausibility of precisely this possibility. Bandura states his view of the nature of modeling and observational learning in the form of a contrast with the direct reinforcement position:

In the social learning system, new patterns of behaviour can be acquired through direct experience or by observing the behaviour of others. The more rudimentary form of learning, rooted in direct experience, is largely governed by

the rewarding and punishing consequences that follow any given action...[But]...virtually all learning phenomena resulting from direct experiences can occur on a vicarious basis through observation of other people's behaviour and its consequences for them.²

Modeling is thus learning by observing the behaviour of others and noticing the controlling contingencies of reinforcement, that is, seeing the relation between the model's behaviour, situation (discriminative stimulus), and consequence (reinforcement). Notice, in the first place, the conceptualization of the process in operant terms. But more importantly, observing the behaviour of others could still be regarded as itself a form of complex behaviour. As such, it too has to be explained. The ability to hold back and see what happens to others in strange situations has obvious survival advantages. Accordingly, it is not at all implausible to suppose that such a complex act-type or ability first arose through the differential reinforcement (selection) of successive approximations. I am not so concerned with doing speculative science; the point is that there is a clear conceptual possibility here of maintaining that in spite of the distinctiveness of modeling as a form of learning, the more basic direct operant conditioning mechanism may be able to serve as a foundation for understanding modeling.

I shall now leave this suggestion and turn to the alternative functions Bandura ascribes to reinforcement.

In addition to the usual response strengthening function, reinforcement is alleged to have an "informative" and a "motivational" function. Bandura quite rightly rejects, I shall argue, a very widespread behaviourist point of view: "It is commonly believed that responses are automatically and unconsciously strengthened by their immediate consequences."³ But I shall also argue that Bandura's manner of supporting this contention is misleading in that it suggests an unwarranted degree of divergence from operant theory as I interpret it. Specifically, I shall attempt to show that Bandura is mistaken in attributing the above functions to reinforcement simpliciter rather than contingencies of reinforcement. Another result that will emerge is that this error leads to a deceptive impression of the degree of difference between human and animal operant behaviour. I shall then reconstruct the sound point Bandura is making in less misleading terms.

The foundational relevance of this matter should be apparent. If both social learning theory and human behaviour diverge from operant theory and animal behaviour respectively as radically as is suggested, perhaps unwittingly, by Bandura, then the role of operant theory will be severely limited.

First of all it should be noted that Bandura does not deny that direct operant conditioning occurs at the human level:

Simple performances can be altered to some degree through reinforcement without awareness of the relationship between one's actions and their outcomes. However, man's cognitive skills enable him to profit more extensively from experience than if he were an unthinking organism.⁴

Radical behaviourists to the contrary, this claim should be regarded as being so obvious as to be almost truistic. The issue has to do with the interpretation of this fact. For the moment I shall forego discussing awareness in particular and simply re-emphasize that even Watson, not to mention Skinner, recognized the role of covert processes. Since the possibility of an operant interpretation is open, there is little ground for regarding Bandura's point as consisting of a radical divergence from operant theory.

Another comment on this passage is in order. I argued that operant theory is more general than Goldman's theory at least in its ability to account for habitual behaviour as well as in its ability to qualify the account of non-habitual behaviour. Here again, despite the lack of philosophic interest in anything but full-blooded self-directed behaviour, it is hard to deny that a good deal of human behaviour is subject to direct conditioning. Accordingly, if I can establish the plausibility of an operant account of the role of cognitive processes then, again, the generality of operant theory is apparent.

For Bandura, cognitive processes come into play by virtue of the other functions of reinforcement, especially the informational function:

During the course of learning, people not only perform responses, but they also observe the differential consequences accompanying their various actions. On the basis of this informative feedback, they develop thoughts or hypotheses about the types of behaviour most likely to succeed. These hypotheses then serve as guides for future actions. Accurate hypotheses give rise to successful performances, whereas erroneous ones lead to ineffective courses of action. The cognitive events are thus selectively strengthened or disconfirmed by the differential consequences accompanying the more distally occurring overt behaviour.⁵

Before I comment on the informative function of reinforcement, it is imperative to see the fundamentality of the direct reinforcement process. I see no reason in principle why the cognitive events Bandura mentions cannot be regarded as covert operant types which may have been acquired by direct differential reinforcement. Furthermore, although particular covert act-tokens will be causally related to particular overt act-tokens, as Bandura himself argues, the covert act-types will be selectively strengthened, shaped, modified, ultimately by the consequences contingent upon the cognitively caused overt act-tokens. Thus we see the fundamentality of operant theory and that the extent to which Bandura diverges from operant theory is slight. This is not meant to downplay Bandura's very real contributions; I am simply attempting to support the theory of scientific change I have adopted.

I shall now argue that it is misleading to attribute an informational function to reinforcement simpliciter.

This will serve to show the gradual character of the transition from animal and human habitual behaviour to deliberative action. The point can be made in the context of an example. If a person were blindfolded and could not see in which situations he was being reinforced, he would obtain useless information. Accordingly, Bandura fails to mention that reinforcement always occurs in the presence of discriminative stimuli; it is the latter that are informative. More precisely it is the contingencies of reinforcement that inform. Or in ordinary language, we are informed by seeing that reinforcement is contingent upon such and such an act-type in such and such a specific situation-type. Hence it is the relation between these three terms that is informative. But now it should be apparent that all organisms, not just man with his cognitive faculties, are "informed" that in the presence of discriminative stimulus y a token of type A will yield x. This unfortunate equivocation on Bandura's part makes his position seem to diverge more widely from my interpretation of operant theory than is warranted.

In simple animal conditioning and human habitual learning situations, therefore, direct exposure to contingencies of reinforcement results in information, response strengthening (act-type formation) and response motivation (increased likelihood of response). Rats, for instance, are "informed" that lever pressing in the presence of y

will lead to reinforcement. Bandura should have spelled out his view on the role of cognitive processes rather as follows: In simple situations, such as the one just mentioned, contingencies of reinforcement operate in such a manner that their various functions are in fact inseparable. It is their inseparability rather than the mere response strengthening function of reinforcement simpliciter that should be emphasized in these simple situations. Such behaviour is shaped or strengthened in the process of "informing" the organism of the appropriateness of certain operant-types.

Bandura wants to draw attention to the role of cognitive processes in adult human learning. His point is better expressed by the assertion that in adult human learning, it is possible for the various functions of contingencies of reinforcement to operate in separation from each other. Thus they can have an informational function totally independently of any response strengthening effects. Accordingly, we cannot read Bandura as saying that contingencies have an informational function as well as a motivational or response strengthening function--as though this had been denied--but rather as saying that in some situations the former can operate without the latter.

Bandura's sound point concerning the role of cognitive processes can now be expressed as follows: The consequences

of behaviour need not always be seen as automatically strengthening it because agents may have conflicting internal states. An agent may have a response reinforced in the presence of y , but instead of a direct increase in the likelihood of similar acts in similar situations the agent may have to deliberate a great deal due to conflicting wants and beliefs (ψ 's and ϕ 's) after which he may actually be less likely to perform the act again. In such a case the reinforcement contingencies have no response strengthening effects, direct or otherwise, they merely inform the agent that certain act-types in such and such situations are characteristically followed by certain consequences.

Accordingly, Bandura's innovation involved an elaboration upon the way in which particular properties of reinforcement contingencies relate to cognitive processes. Once it is seen that contingencies can be said to have the three functions already that Bandura attributes to them, his divergence from operant theory no longer appears so radical. And once it is seen that these three functions play a role vis-à-vis animal behaviour as well, the difference between animal behaviour and human behaviour, as far as the foregoing matters are concerned, turns on the presence of the cognitive dimension in the latter rather than on the different functions of reinforcement contingencies as well. Explanatory power is increased by virtue of

our greater understanding of why and how behaviour can be acquired with or without cognitive mediation. And if cognitive processing is an acquired ability, the more primitive operant theory is fundamental. The result is in accordance with both the theory of scientific change relied upon herein and with Bandura's own view of his work as pertaining to the filling but of an incomplete operant picture.

Although I have mentioned the motivational function of reinforcement contingencies in the foregoing discussion, I have concentrated on the informational function. Since Bandura's comments on the former are also seriously misleading, I shall have to criticize them briefly as well. Consider Bandura's statement of the motivational function

Because of man's anticipatory capacity, conditions of reinforcement also have strong incentive-motivational effects. Most human behaviour is not controlled by immediate external reinforcement. As a result of prior experiences, people come to expect that certain actions will gain them outcomes they value, others will have no appreciable effects, and still others will produce undesired results. Actions are therefore regulated to a large extent by anticipated consequences.^{6a}

I have stressed the reference to man and human behaviour in this passage to call attention to the possibility of misunderstanding arising from reading Bandura's claim as pertaining to man alone. For in the simplest organisms susceptible to

operant conditioning techniques, it must be the case that certain structural changes are consequent on reinforcement such that organisms "remember" and "expect" the continuation of the relation between operant-type, discriminative stimulus, and reinforcement.

This is especially obvious in the case of animals on intermittent schedules of reinforcement where it will clearly be appropriate to postulate some mechanism at least resembling expectancy in order to account for continued responding during periods between reinforcement occurrences. Accordingly, it is surely misleading to suggest that it is only the actions of humans that are "regulated to a large extent by anticipated consequences". What should be said to differentiate animals from humans is not anticipated consequences simpliciter but the ability of humans to cognitively calculate possible-to-be-anticipated long range consequences.

I don't mean to imply that Bandura is deliberately suggesting such differences between animals and man. But, the possible reading of the above passage I discussed shows that Bandura's comments are at best sloppy, especially when he also discusses the cognitive sense of anticipation

I suggested:

Through the capacity to represent actual outcomes symbolically, future consequences can be converted into current motivators that influence behaviour in much the same way as actual consequences.

The possibility of accounting for such cognitive abilities in terms of direct operant conditioning should be kept in mind.

The upshot of these critical remarks on Bandura's discussion of the informative and motivational functions of reinforcement contingencies is clear. Without contrasting human and animal operant processes, Bandura conveys the impression, likely unwittingly, that he is postulating mechanisms that are totally out of place in operant theory as it pertains to animal behaviour. Once the degree to which Bandura's social learning theory diverges from operant theory is clarified, his contentions can be seen as valuable contributions, not to the overthrow of operant theory, but to its development via the introduction of some detail concerning the very real place of internal states and cognitive processes in human behaviour.

The foregoing has been a discussion of the differential effects of observing the consequences of one's own behaviour. I shall now turn to a more detailed consideration of the results of observing the consequences of the behaviour of others: the process of learning by modeling. I shall argue that Bandura's work on this process constitutes an instance of paradigm articulation where the latter is viewed as involving the making of slight changes in the original paradigm as opposed to a mopping up operation as Kuhn views paradigm articulation. It is surely an operant

explanation of a process Skinner has alluded to and labelled imitation.

In line with the evolutionary theory of change I have adopted, it is imperative to point out that the following claim of Bandura's can also be found in an analogous form in Skinner's writings:

Although behaviour can be shaped into new patterns to some extent by rewarding and punishing consequences, learning would be exceedingly laborious and hazardous if it proceeded solely on this basis.⁸

Bandura is surely correct if he means to imply that earlier behaviourists such as Skinner have over-emphasized the role of direct and explicit shaping of behaviour through the differential reinforcement of successive approximations. But there is more continuity between Skinner and Bandura than emerges from a reading of Bandura alone as we shall see shortly.

The question of the status and role of modeling ties in nicely with the question of the role of cognitive processes.

Opponents of the narrower direct reinforcement view can approach both questions from a common vantage. That is, it can be argued that humans can avoid subjecting themselves to the actual consequences of their own behaviour in either one of two ways: either by in some way cognitively calculating or estimating the consequences in advance of behaving, or by observing the consequences of the behaviour

of others.

With respect to both of these very real and important ways of circumventing the direct operant conditioning process, it can be maintained that the concept of a reinforcement contingency is still the central theoretical notion in explaining the acquisition and maintenance of these abilities. This concept is at least important in understanding the acquisition of particular cognitive and modeling patterns. In any case, it seems correct to maintain that whatever causes us to behave seems to be contingencies of reinforcement operating in some fashion, whether we experience them directly, consciously or unconsciously, calculate them in advance, or observe their effects on others. Regardless of the circumvention of unconscious direct operant conditioning, the centrality of the concept contingency of reinforcement can be seen in that it is contingencies that we are exposed to, calculate, or observe.

When we turn to the continuity in the behaviourist tradition, it is instructive to notice that even Watson recognized the role of covert processes. As we have seen, Heidbreder conveys Watson's view of the function of thought in man as one of "the power of manipulating his environment without making the actual overt movements."⁹ Skinner has discussed modeling or imitation as well:

It is possible to use techniques based upon reinforcement and punishment without being able to control the events in question. A considerable effect may be achieved by clarifying the relation between behaviour and its consequences. The instructor in sports, crafts, or artistic activities may directly reinforce the behaviour he is trying to establish, but he may also simply point up the contingency between a given form of behaviour and the result--"Notice the effect you get when you hold the brush this way," "Strike the key this way and see if it isn't easier..."¹⁰

It is noteworthy that this statement was published in 1953 and is therefore not a recent change of view. Clearly the instructor in Skinner's examples is demonstrating the behaviour that has the requisite reinforcing consequences. And this is precisely what Bandura calls modeling. Again, in my discussion of the relation between rules and reinforcement contingencies, it was shown that Skinner's account of why and how communities or groups extract rules is precisely because it is reinforcing to do so--that is, because it is easier to state a rule than it is to shape behaviour explicitly once the learner has grasped the relation between rules and reinforcement contingencies. Clearly, therefore, Skinner recognizes and tries to explain the behaviour of avoiding the use of explicit conditioning.

Thus Bandura's point that,

...it is difficult to imagine a socialization process in which the language, mores,...of a culture are taught to each new member by selective reinforcement of fortuitous behaviours, without benefit of models...¹¹

is misleading if it is taken to suggest that someone actually holds this view. The point of these critical remarks is neither to devalue Bandura's contribution nor to elevate Skinner to the level of having said everything. On the contrary, the sort of search for continuity in the history of science that much of the present inquiry exemplifies, is typified by attempts to show that apparently revolutionary innovations are just that: apparent.

Bandura is thus not saying anything startlingly new nor radically rejecting operant theory. Scientific change is a slow and laborious process and Bandura's work can be viewed as a straightforward instance of paradigm articulation: elaboration of detail, experimental exploration, and modification and extension of operant theory.

Accordingly, although Skinner recognizes (perhaps dimly) that agents can acquire new act-types observationally, Bandura has provided us with a detailed analysis of the process. I shall now make a few further brief comments on Bandura's contentions before turning to other issues in the foundation of learning theory. Again the point will be to show the existence of continuity.

Further comments are required because Bandura occasionally conveys the impression that his social learning theory is in direct opposition to the direct reinforcement view. Referring to the latter he claims that it

...does not explain how a new matching response is acquired observationally in the first place... such learning occurs through symbolic processes during exposure to the modeled activities before any responses have been performed or reinforced.¹²

There are two comments to be made here. First, without a comparison with animal modeling we are left without any idea of the specific role in modeling that symbolic processes are supposed to play. Second, Bandura appears to be suggesting that observational learning is more

fundamental than operant conditioning. But, again, without animal comparison, such a claim is entirely without any force. Although Bandura did say that early behaviourist theories were not wrong, just incomplete, there is further evidence that he does want to oppose his theory to those of earlier behaviourists. Referring first to the latter and then to his own theory he states that

Since, in this view, behaviour is organized into new patterns in the course of performance, learning requires overt responding and immediate reinforcement. According to social learning theory, behaviour is learned, at least in rough form, before it is performed.¹³

Why the opposition here? Bandura himself points out in several places that learning can take place in either way and that, in actual socialization contexts, it is probably a mixture of the two processes, modeling and direct reinforcement. Evidence of the unsettled state of this issue

and of Bandura's lack of divergence from operant theory is available elsewhere. He there summarizes his view of the matter as follows (vicarious reinforcement refers to observed consequences of a model's behaviour):

Evidence supporting the relative superiority of vicarious reinforcement should be accepted with reservation for several reasons. The experiments on which this evidence is based simply require subjects to learn to discriminate responses that already exist in the repertoires of observers and performers. Consequently, selective vicarious reinforcement mainly serves an informational function in helping observers to identify the types of responses that bring rewards or punishments. As previously noted, this form of discrimination learning is apt to be hindered rather than aided by overt performance. However, on tasks involving the acquisition of new complex skills, reinforced performance would probably prove more efficacious than observation alone, particularly in response learning that requires abstracting subtle common properties from otherwise different instances and in developing skills containing important motoric components. It should be also be noted that the studies have not demonstrated that vicarious reinforcement alone can sustain effortful behaviour over a long period, which is usually the main function of direct reinforcement...The overall findings indicate that vicarious reinforcement alone can have strong short-term behavioural effects. Moreover,...observation of other people's outcomes can have a continuing influence on direct reinforcement by providing a standard for judging whether the reinforcements one customarily receives are equitable, beneficent, or unfair.

Since both direct and vicarious reinforcement inevitably occur together under natural conditions, the interactive effects of these two sources of influence on human behaviour are of much greater social significance than their independent controlling power.¹⁴

In those parts of the above passage that I have emphasized, it is clear that Bandura has concluded in favor of the fundamentality of the direct reinforcement process vis-a-vis the acquisition of new operant-types. It is conceivable that organisms are able to learn by modeling only after they have acquired some operant-types via the direct reinforcement process by grasping tacitly the relation between behaviour and contingencies of reinforcement. In any case this matter is an unsettled issue at the foundations of learning theory and this is hardly the place to settle it. For my purposes it is sufficient to note the causal interactive nature of the two processes and the fact that they can both be conceptualized in operant terms relying foundationally on the concept of a reinforcement contingency. It is also a suggestive possibility that cognitive mediation in both direct reinforcement and modeling is a set of act-types acquired through operant conditioning. This is an empirical matter, however; I simply call attention to the theoretical plausibility of the contention.

Other internal mechanisms are postulated by Bandura without any comment on whether they exist in animals as well. Among them are attentional and retention processes. But it must be emphasized that these processes are operative in all operant conditioning. Skinner's failure to deal with them reflects his personal atheoretical bias.

rather than the incompatibility of operant theory with the existence and causal role of such processes. Thus we can view Bandura's discussion of these processes as involving the necessary filling out of the incomplete picture he alluded to himself.

I shall close my discussion of this foundational issue with some brief comments on the claims of a more radical behaviouristic writer on modeling and observational learning. In his critical comments on Bandura's work, Jacob L. Gewirtz attempts to interpret the modeling process within a more narrowly behaviourist framework than I have done in the foregoing.¹⁴ Gewirtz argues that observational learning depends to a greater extent on relevant prior learning (history of reinforcement), than Bandura's exposition leads us to believe. The relevance of prior learning is such that:

A predictable outcome of acquired stimulus control might appear to be an instance of rapid, often instantaneous learning through observation to those unfamiliar with an organism's learning history.¹⁵

In other words, stimulus generalization might play a prominent role in observational learning so that the latter would simply amount to an already existing response occurring perhaps in a slightly novel form due to the agent's seeing it reinforced in a slightly novel context. This would correspond to the informational function that,

for Bandura is the most significant role played by vicarious reinforcement (observed consequences of a model's behaviour). But, again, this is rather speculative and is designed to show no more than that the issue is far from settled. Nonetheless, it seems to me that, as I have argued, it is not at all implausible to suppose that both forms of learning can be accounted for in operant terms.

I shall now consider two further issues of relevance to the foundations of learning theory. One question pertains to the nature of what has been called self-reinforcement. My comments will be very brief; I shall simply mention the fact that there does seem to be a plausible operant theoretic concept of self-direction or self-control. Skinner has discussed self-control, but again it is viewed as itself behaviour to be explained in terms of history of reinforcement and prevailing contingencies of reinforcement. Bandura and Gewirtz also discuss self-reinforcement and I shall very briefly touch on their views. One other behaviourist source I shall, again simply mention, is that of David L. Watson and Roland G. Tharp. In a recent book entitled Self-Directed Behaviour,¹⁶ these authors develop the vague comments on the nature of self-control made by Skinner and Bandura into a systematic operant account.

They state their aim as follows:

The most important goal of this volume is to help you, the reader, achieve more self-determination, more "will-power", more control over your own life.¹⁷

One would have thought that nothing could be more diametrically opposed to a behaviourist theory than something resembling the power of positive thinking. It would thus be most ironic if a "behaviourist" formulation of this notion turned out to constitute its most adequate expression. Of course, such an account could be considered behaviouristic in its historical origins only.

Naturally Skinner's vague account of self-control comes to no more than the claim that an agent is able to control his own behaviour "through the manipulation of variables of which behaviour is a function."¹⁸ But since even in exercising such self-control, an agent is still behaving, his "behaviour in so doing is a proper object of analysis and eventually it must be accounted for with variables lying outside the individual himself"¹⁹ It should be intuitively clear how these vague hints would be developed using the interpretation of operant theory I have suggested. As with the modeling process, Bandura's most significant advance over Skinner on the investigation of self-control or self-reinforcement is simply an elaboration on detail that is based on experimentation that Skinner never carried out.

I have chosen to allude to this topic solely to

increase the intuitive sense in which operant theory can be said to have great generality as well as to further indicate that the theory is not narrowly mechanistic. How the resulting concept of self-determination relates to philosophic views of this concept is a topic that deserves serious and detailed study. It should be apparent that if operant theory can actually explain the acquisition of the ability to exercise a high degree of self-control and self-determination then there would be virtually no significant obstacles in the path of its succeeding commonsense action theory.

There is one further topic that lies at the foundations of learning theory that I shall discuss that has been receiving a great deal of attention of late, and that is the role of awareness in conditioning and learning generally. I shall attempt to show that the issue is largely a pseudo-problem once the narrower form of operant theory is rejected in favour of the one suggested in the present inquiry. For the purpose of showing just how the more positivistic aspects usually associated with all behaviourist theories underlie much of the dispute over awareness, it will be convenient to discuss the views of a Skinnerian that is at least as positivistic in outlook as Skinner himself. I have in mind one of the operant leaders of the "absence of awareness position", Leonard Krasner. I shall

consider four alternate definitions of awareness that

Krasner discusses. The first is as follows:

1. Awareness may be viewed as a mediating response which occurs under specific stimulating conditions and, as such directly affects learning. There may be a direct relationship, i.e. the learning of the task may be a complex interaction involving awareness and other variables, such as the subject's intentions. However phrased, learning does not take place unless the subject can correctly verbalize the contingencies.²⁰

I find the criterion of awareness proposed in the last sentence above unfortunate. Surely there are degrees of awareness. Kuhn and others have documented cases in the history of science where scientists only gradually became fully enough aware of what was going on in experimental contexts to verbalize what they were doing. Yet clearly they were aware in some sense of what they were doing. They were not behaving randomly; their experimentation was not something happening to them.

Surely there are also degrees of learning. One may learn a task to a certain degree of--perhaps pre-verbalizable--mastery under the direct control of reinforcement contingencies without being very aware at all of what the task is all about. It may be the case that full awareness in the sense of being able to verbalize the contingencies mediates only the very final stage of learning where one fully masters the task. This possibility is in line with a more evolutionary account.

and suggests that we need not view the matter in such an all-or-none fashion as seems to be suggested by Krasner. It may be neither the case, therefore, that all learning proceeds without awareness, nor that none so proceeds.

I shall now comment on the next two views of awareness Krasner adumbrates:

2. Awareness may be independently affected by different antecedent conditions than the acquisition or learning of the specific task. This position would argue that awareness and learning are unrelated.

3. Awareness is a function of the same sets of variables as acquisition of the response class and thus represents a separate experimental event that may well be correlated with the learning behaviour, i.e. awareness and learning may be related, but this is fortuitous and one is not mediating the other.²¹

The second view seems so intuitively implausible that I shall not consider it. But the third view has a familiar ring. As we have seen, some of Skinner's arguments for the explanatory powerlessness of mental entities seemed to suggest that the latter were mere by-products of the conditioning process. On the other hand, if the interpretation of operant theory sketched in the foregoing is at all plausible, then it should be possible to construct a reasonable view of the role of awareness that is both consistent with my interpretation of operant theory and at least close to views 1. and 3. above. Of course the actual role of awareness in learning is an empirical

matter. Accordingly, I intend merely to suggest one view of awareness that would be consistent with operant theory as I have sketched it. Indeed, the view I have already presented in commenting on the first view of awareness listed by Drasner can be employed to modify the third view. In these comments I suggested that learning proceeds by degrees, by the differential reinforcement of successive approximations. Primitive forms of awareness thus mediate early stages of learning, so that, contrary to the first view above, only the final stages of the learning of a complex task is mediated by full-blown awareness of the contingencies.

One can see the evolutionary perspective in this view. One explains the "chicken and egg" dilemma away by pointing to the evolutionary development of both by a process of emerging successive approximations to their present form so that one is not committed to maintaining that either the present day egg or the present day chicken came first. A similar account of learning is consistent with operant theory and captures some aspects of the first and third views of awareness Krasner discusses. As far as the third view is concerned we can say that both awareness and learning are "functions of the same sets of variables" without saying that the relation between the two is fortuitous. Just as both egg and chicken are functions of natural selection and not fortuitously related, so awareness and learning are functions of differential reinforce-

ment with each stage of learning and awareness being necessary for further development of both. It is illuminating to keep in mind the fact that, on this account, since we would only be aware of the final stages of acquisition of a complex act-type, we would not be cognisant of the earlier stages of learning or awareness. No wonder we think that full-blown awareness mediates all learning.

It is very easy to see the extent to which Krasner's statement of the above three views on awareness is affected by the older S-R type of behaviourism as opposed to the evolutionary aspect of operant theory I have emphasized. If each R were a direct effect of an efficient cause S, then either awareness would be a link in the chain or S would cause both awareness and R. There is no room even recognized here for any talk of developmental interactional stages.

The fourth view of awareness amounts merely to a suggestion of the possibility of inadequate experimental design so I shall not consider it. Instead, I shall comment on Krasner's discussion of the cognitive approach to verbal conditioning. After quoting Spielberger's summary of the cognitive approach, Krasner states that:

A major distinction, then, between the cognitive and the behavioural approach to verbal conditioning, is in the nature of what is learned by the subject. The cognitive group infer that the subject has learned a correct hypothesis about

the nature of the relationship between the reinforcing stimulus and his own verbal response. Whether or not he acts upon this learned hypothesis depends on his motivation to receive reinforcement; whereas for the behaviourist, learning is defined in terms of a change in performance on the given task, i.e. performance is learning and vice versa.²²

But clearly the behaviourist definition of learning, at least as Krasner envisages it, is absurd. It is difficult to see any point to such a narrow conception of learning once positivist and operationist scruples are rejected.

But of course once learning is viewed as alteration of internal structure one is faced with at least the possibility of modifying such structure in a manner other than direct conditioning. Again, however, as I have been at pains to argue, it could still be the case that the theoretical role of the concept of a reinforcement contingency could be central. That is, even if there were pure observational learning prior to reinforced performance, it still might be the case that such learning could be regarded as resulting from exposure to contingencies of reinforcement. In such a theory, the latter concept would be foundational, but there would be room for various forms of exposure to contingencies of reinforcement such as were discussed earlier.

Moreover, is it even worthy of consideration that learning is nothing but performance change? Once the absurdity of this conception is recognized and once the

evolutionary aspect of operant theory is fully understood, it emerges that the learning with or without awareness dispute is quite empty. It is ridiculous enough that Krasner regards all learning as performance change, but even more obviously false is the notion that all performance change is learning. Simple generalization would count as performance change but not learning.

As I have interpreted learning, act-type changes are identical with ability changes. In the course of learning, both change regardless of whether contingencies of reinforcement operate directly on act-tokens or directly on internal states. In any case, learning would be conceived as possible in the absence of performance. Furthermore, even in simple animal conditioning it is not unjustifiable to say that, due to exposure to reinforcement contingencies, organisms learn that a particular connection obtains between behaviour, situation, and consequence. This roughly corresponds to the cognitivist's view that learners learn the "correct hypothesis about the nature of the relationship between the reinforcing stimulus and... response." The difference would be simply the absence of cognitive events in the case of the animal's "hypothesis".

Nor is there anything absurd about saying even of an animal that "Whether or not he acts upon this learned hypothesis depends on his motivation to receive reinforcement." For animal subjects, if they are satiated, will

not be "motivated" to "act upon the learned hypothesis".

So Krasner's discussion of the controversy over the nature of what is learned fails to come to grips with even the most basic issues. The narrowly behaviourist position is indefensible both with respect to the nature of learning and with respect to the role of awareness and other internal states and processes.

In order to see as clearly as possible how the present interpretation of operant theory is consistent with the admission of cognitive processes it may be useful to take a brief look at Spielberger's view of the foregoing issue. Krasner reproduces the following passage from Spielberger:

(a) Cognitive processes such as thoughts, ideas and hypotheses exist. (b) Although cognitive processes are not directly observable, they may be inferred, albeit imperfectly, from subjects' verbal responses to interview questions... (c) Cognitive processes are lawfully related to antecedent conditions... as well as to consequent changes in verbal behaviour... (d) Cognitive processes mediate performance gains in verbal conditioning by permitting the selection of those responses which lead to reinforcement, ... 23

It is difficult to conceive of any legitimate behaviourist objection to such theoretical postulation. If the behaviourist scruples have any permanent methodological worth, it may be in keeping speculation about covert processes as rigorous and testable as possible. But as virtuous as this sort of rigor is, it hardly constitutes grounds for the dismissal of real processes. Once we

reject observability as the sole criterion of theoretical legitimacy, the only positive role for behaviourist methodology is in the provision of human and animal comparative studies. But now there is nothing behaviouristic about such comparison.

Moreover, Skinner himself does not refrain from discussing covert processes. Although it is undeniable that he minimizes their role, he comments on their causal efficacy in a number of places. His most explicit statement pertaining to this matter is contained in his most recent work where he says:

Covert behaviour has the advantage that we can act without committing ourselves; we can revoke the behaviour and try again if private consequences are not reinforcing...Covert behaviour is almost always acquired in overt form, and no one has ever shown that the covert form achieves anything which is out of reach of the overt.²⁴

Despite terminological differences, this statement appears to resemble Spielberger's view very closely--especially point (d). Both psychologists agree that there are covert/cognitive act-sequences, the alternative estimated consequences of which, are causally relevant to the ensuing overt behaviour. On this view, such cognitive processes are not mere links in causal chains beginning with current environmental stimulation. Rather, they would seem to constitute causal processes themselves in as much as their occurrence makes a difference to the resulting

behaviour over and above mere communication of external contingencies.

After the above assertion, Skinner goes on to criticize the positivistic position he calls methodological behaviourism for choosing to ignore covert behaviour. In his view, covert verbal behaviour

...is far from an adequate substitute for traditional views of thinking. It does not explain behaviour: it is simply more behaviour to be explained.²⁵

Without the interpretation of operant theory earlier sketched, this statement is apt to appear quite mysterious. The claim is simply that covert behaviour or cognitive processes still are act-types or sequences of tokens of such. There is thus no in principle objection to the inclusion of such processes as mediators vis-a-vis overt act-token occurrences and even learning in sophisticated learners. Skinner's operant theory may be mistaken but it is not, as I have interpreted it, positivistic. To the extent that the existence and causal role of such processes can be accounted for along evolutionary lines, and to the extent that operant theory is, or is like, the theory of evolution, then operant theory is fundamental with respect to the explanation of behaviour.

There does appear, however, to be a problem of "subjectivity". If awareness and cognitive processes

mediate between the reception of reinforcement contingencies and structural changes and again between the latter and overt behaviour, what sense is there to the claim that ultimately it is the objective contingencies that are responsible for all act-type generation, overt or covert, direct or indirect? In other words are not consciously rational agents, in particular, fully in control of their behaviour since they must first interpret the contingencies?

Although I think we have a degree of freedom that is greater than species have over the course of their own evolution, nonetheless, the same sort of "subjectivity", if it exists at all, obtains with respect to the theory of evolution. As species become increasingly complex, the causal effect of the selective consequences of alternate variations is going to undergo an increasing amount of "interpretation" before inducing type change. Because of the extreme complexity of such species there will be many more internal causal conditions that will play an increasingly prominent role in combination with the consequences of the variations themselves. But surely this does not render the real selective role of the environment itself "subjective". It is possibly only our ego-centric reliance on the "evidence" of immediate awareness that blocks our ability to view operant theory in the same way.

I have now illustrated my interpretation of operant theory by showing what it implies for significant issues in

the foundations of learning theory. Before turning to a similar project with respect to the foundations of the social sciences I shall attempt to show how certain standard criticisms of operant theory, those of Chomsky, dissolve in light of the present view of operant theory. Since Kenneth MacCorquodale has replied to Chomsky in a more traditional behaviourist manner it will be necessary to disavow some of his arguments as well. Crucial epistemological and methodological matters that are raised by Chomsky receive treatment by MacCorquodale that amounts to little more than a re-statement of standard logical empiricist doctrines. On the other side of the fence it would appear that Chomsky's view, at least as far as general epistemological and methodological issues are concerned, is shared by Spielberger and Dulany among many others. The discussion of the latter writers of the role of awareness in verbal conditioning is thus not a purely empirical matter but rather the battleground for large philosophical issues.

I shall begin by distinguishing those aspects of MacCorquodale's reply to Chomsky I consider adequate from those I take to require deeper consideration. Undoubtedly MacCorquodale does succeed in clearing up some confusions; he is certainly right to dismiss Chomsky's attack on the drive reduction theory of reinforcement since it "has long since disappeared from everyone's behaviourism," and since it "never characterized Skinner's."²⁶ The bulk of MacCorquo-

dale's reply consists in a consideration of "three basic methodological criticisms".²⁷

The first such criticism is stated by MacCorquodale as follows: 1. "Verbal Behaviour is an Untested Hypothesis Which Has, Therefore, No Claim upon Our Credibility."²⁸ It must be noted that this is one of Chomsky's major criticisms of the extension of operant theory from animals to humans according to MacCorquodale. Now regardless of the weight Chomsky places on it, there is clearly something to be said both in its favor and against it. On the one hand, much experimental and theoretical research is required before operant theory's applicability to complex human behaviour can be fully assessed. On the other hand, such an obvious fact hardly suffices to condemn the very approach as though it were fundamentally incoherent.

In any case, one suspects that Chomsky does not place a great deal of weight on this type of criticism at all. Chomsky did not intend to suggest that Skinner was offering a merely untested hypothesis, but one that was founded on methodological and epistemological presuppositions that were thoroughly misguided. And surely there is more justification for Chomsky's strategy than there is for MacCorquodale's, for it is stretching the term "hypothesis" to call Skinner's claims hypotheses, untested or not. That the laws discovered (more accurately, to be discovered!) in the study of animal behaviour are extendable without substantial modifi-

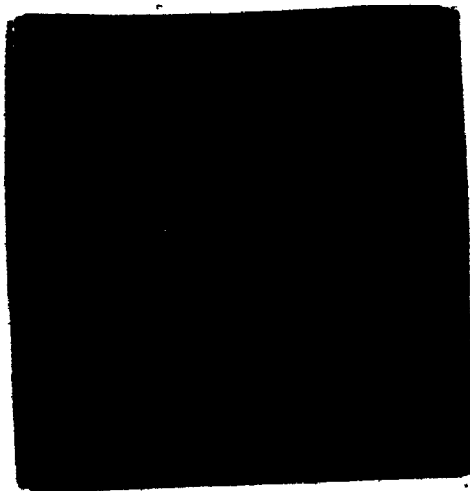
cation to cover human verbal behaviour is not a hypothesis but a programme. It is thus somewhat irrelevant to point out, as MacCorquodale does, that although "the hypothesis may prove wrong,"²⁹ our confidence in it is enhanced by the present sophistication of animal research. Unless one faces the fundamental issues involved, one is merely re-asserting the statement of the programme with very little, if any, cash provided for Skinner's original promissory notes. On the other hand, MacCorquodale is entirely justified in claiming that Chomsky has groundlessly asserted the incoherence of Skinner's very approach. Unfortunately, MacCorquodale does not see that the reason for Chomsky's failure to demolish operant theory is that the latter can (and must if it is to survive) be reformulated within a more adequate methodological and epistemological framework. Before discussing important issues pertaining to the latter, I shall comment briefly on MacCorquodale's replies to what he considers Chomsky's other two major criticisms.

Very little space need be wasted on MacCorquodale's discussion. The main point of touching on it at all is to show how completely different the present perspective on evaluating operant theory is from MacCorquodale's. The second of Chomsky's criticisms, in MacCorquodale's view, is this: "Skinner's Technical Terms are Mere Paraphrases for More Traditional Treatments of Verbal Behaviour,"³⁰ Clearly, if the bulk of my argument thus far has had any force at

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all, it is impossible either to make this criticism stick or to refute it without an adequate and detailed theory of scientific change. Indeed, much of the discussion of Chomsky's criticisms will be to show just how important such a theory is in this area. There may be a risk of repetition but the acceptance of Chomsky's "refutation" of operant theory is far too widespread to forego commenting on it. MacCorquodale's reply to this second criticism of Chomsky's is as follows:

Although Skinner did not do so, it probably would be a service if a scientific and technical paraphrase were given for such traditional mentalism as 'refer', 'denote', 'meaning', 'wanting', 'liking', and so forth...31

We have already seen that Skinner feels that such efforts would be a waste of time. Physicists are not required to produce paraphrases of "phlogiston". But notice that MacCorquodale fails to reply adequately to Chomsky in any case.

If the charge is that operant theory is a mere paraphrase of a traditional theory, it will hardly do to provide a more detailed paraphrase. Without a theory of scientific change embodying some such principle as the GCP, Chomsky and MacCorquodale are just talking past each other. It must be understood that successor and predecessor theories converge to the point of trivial transition while on the whole the successor theory is broader in scope, has more explanatory power, etc. than its predecessor. The

status of the mentalisms cannot be determined by translation. Operant theory must develop its own theoretical terms. Comparison of the latter with the terms MacCorquodale mentions is the only way we will be able to ascertain at which point the two theories converge into one.

The third criticism is as follows: "Speech is Complex Behaviour Whose Understanding and Explanation Require a Complex, Mediational, Neurological Genetic Theory".³² Now regardless of whether this statement does justice to Chomsky's intentions, it is clearly too nebulous to have much force. (Part of MacCorquodale's reply to this criticism consists in the justified point that Chomsky overlooks Skinner's discussion of "multiple causation" when arguing that it is impossible to believe that complex verbal behaviour is the result of "a single simple function"). But the aspect of MacCorquodale's reply that I should like to take issue with, pertains to mediational processes.

According to Chomsky:

One would naturally expect the prediction of the behaviour of a complex organism (or machine) would require, in addition to information about external stimulation, knowledge of the internal structure of the organism, the ways in which it processes information, and organizes its own behaviour.³³

In reply, MacCorquodale argues that one need merely know that internal structure functions lawfully (a typically Skinnerian reply):

it is not necessary in the least to know how the internal structure goes about doing so, nor which structures are involved...It is simply false, of course, that one cannot accurately predict behaviour, even complex behaviour, without knowing and taking into account the behavior's structure and internal processes; we do it all the time.³⁴

Obviously the latter claim is completely wide of the mark. From the fact that we "predict behaviour all the time" it does not follow that we have done so on the basis of behaviour and reinforcement contingency observation alone. It would have to be shown that we could make such predictions without knowledge or inferences of internal structure.

But the first part of MacCorquodale's reply is inadequate as well. Even if we had a functional law sufficient to account for all structural changes (in spite of covert processes), the law would only tell us how, in general, internal structure is altered. We would still need information regarding the particular contingencies of reinforcement the agent was exposed to in his lifetime. And this means knowing his history of reinforcement, which, as we have seen, is represented by his internal states. Thus Chomsky is right. The fact that internal structure processes information lawfully doesn't tell us what particular information has been processed at a particular time.

But MacCorquodale probably has something else in mind. If we can control for an agent's history of reinforcement,

then we should be able to predict the effects of exposure to current contingencies of reinforcement without knowing how they are processed. If this were true, prediction might be possible, but we would still want a neurological story to give us a complete explanation. On the other hand this is just what is at issue for Chomsky--in which case MacCorquodale's reply just begs the question. For Chomsky would deny that the agent's internal structure does not make an original contribution over and above "input". But on this issue both Chomsky and Skinner are passing out promissory notes. To the extent that this is a methodological issue, therefore, both "hypotheses" need much further testing. MacCorquodale's error here is apparent in a number of places (Cf. p. 92). But the error is more one of simple failure to come to grips with the issue than one of question begging; for it is not even clear what could be meant by "contribution of the organism". MacCorquodale refers to genetic programming and this appears to be what Chomsky has in mind. But in this case there should be no opposition between the two theories.

Opposition arises, historically, from the behaviourist's perverse desire to provide a complete explanation of behaviour in terms of environmental input, as though there were no genetic inheritance at all. MacCorquodale simply misses the point when he states the lawfulness of a genetically pre-programmed mediator. For the issue, to put it crudely, is whether reinforcement con-

tingencies constitute input to the mechanism or whether they simply release information already stored there. Skinnerians argue that the former is the whole truth, Chomsky the latter. But here again promissory notes are all either side has to offer.

I shall now turn to Chomsky's review. As with MacCorquodale, I shall argue that once positivistic doctrines are dismissed, two types of issue remain. On the first there are no substantial methodological and epistemological differences between the two views that can be sustained. And on the second mere promissory notes are passed out.

Commenting on his review of Skinner's Verbal Behaviour after eight years, Chomsky tells us that he does not see how Skinner's "proposals can be improved upon, aside from occasional details and oversights, within the framework of the general assumptions that he accepts."³⁵ In other words, it is not, or not merely, the lack of evidence for Skinner's contentions that Chomsky objects to, but rather the very conceptual and foundational presuppositions of his entire approach. Clearly MacCorquodale misses much of this aspect of Chomsky's attack. I shall argue that Chomsky's critique fails precisely because he does not see any way in which a substantive operant theory can be placed upon more viable foundations. Although I shall endeavour to avoid repetition, some detail is absolutely necessary. It will be illuminating

to see just how Chomsky misses the mark.

In his review of Verbal Behaviour, Chomsky goes astray right from the very beginning where he states what he takes to be Skinner's aim. It will be worthwhile to reproduce this misconceived statement of the aim of operant theory in full, since it involves the central assumptions Chomsky attacks. Chomsky states the goal of Verbal Behaviour as follows:

The problem to which this book is addressed is that of giving a 'functional analysis' of verbal behaviour. By functional analysis, Skinner means identification of the variables that control this behaviour and specification of how they interact to determine a particular verbal response. Furthermore the controlling variables are to be described completely in terms of such notions as stimulus, reinforcement, deprivation, which have been given a reasonably clear meaning in animal experimentation. In other words, the goal of the book is to provide a way to predict and control verbal behaviour by observing and manipulating the physical environment of the speaker.³⁶

A number of fundamental assumptions are here attributed to Skinner and thereby to operant theory. Those that I have already considered will be dismissed very briefly. The most important isolable assumptions that can be extracted from the above passage are as follows:

1. (a) Behaviour is explainable entirely in terms of functional laws.
- (b) A science of behaviour aimed at the establishment of functional laws is possible without consideration of theoretical entities.

2. The variables of which behaviour is a function can be "described completely in terms of such notions as stimulus, reinforcement, deprivation..."
3. Verbal (and nonverbal) behaviour can be predicted and controlled by observation and manipulation of the physical environment.
4. Verbal (and nonverbal) behaviour can be predicted and controlled by observation and manipulation of the physical environment.
5. The ultimate aim of a science of behaviour is prediction and control.

These presuppositions are so basic that they are rightly called assumptions by Chomsky and not hypotheses. Consider, first, the doctrines I have already discussed. As far as 1. is concerned, I have argued that Skinner has primarily advocated 1 (b) as opposed to 1 (a). And in any case operant theory as it has thus far developed has been successful because it has not become bogged down in theory. My contention has been that although there are solid grounds for the continuance of such a theoretical investigation, operant theory can, in principle, postulate theoretical structures that closely correspond to those of current theories of human action.

Nor is operant theory committed to 2. That aspect of the theory that consists of the functional law or laws of reinforcement--which is the only aspect that Skinner has developed, is committed to 2, but this is no restriction

on the future of operant theory. It is noteworthy that Chomsky leaves "history of reinforcement" off the list of "notions" to which he conceives operant theory to be restricted. It will emerge that this omission seriously undermines Chomsky's critique.

Chomsky is quite correct in attributing 3. to Skinner. MacCorquodale also defends the physicalist view, which is not implausible in itself, but which becomes absurd when the restriction is to physical environmental and behavioural observables. In MacCorquodale's words: "Every term in Skinner's account names some real thing which must be physically involved and locatable in any verbal event for which it is invoked."³² But clearly, acts have properties that will not be named in a purely physical description of movement--not, at least, in a narrow observable sense of "physical".

As we shall see upon examination of Chomsky's review, the mistaken attribution of 4. to operant theory and to Skinner vitiates Chomsky's attack to a much more substantial degree than any other single misconception. Although the confusion concerned is largely due to Skinner himself it is clear that no prediction will be possible without complete information about the agent's history of reinforcement. Obviously, mere observation of the agent, his current behaviour, and his situation will not be sufficient to warrant reliable predictions. For most of the properties

that make a particular discriminative stimulus the particular causal agent it is, are due to an individual's history of reinforcement. An agent's internal structure combines with the purely physical stimulus to occasion an operant-token; precisely Chomsky's point. But this is not to concede the incoherence of the totality of operant theory. On the contrary, it is to suggest that substantial foundational and theoretical revisions of the theory are in order--something Chomsky does not even allow.

The strategy of Chomsky's attack consists in an attempt to show that when operant terms are extrapolated from the animal laboratory to complex verbal behaviour they become nothing more than misleading paraphrases for traditional terms. As the GCP indicates, there is some truth to this criticism. But it fails on two counts: It misses the subtlety of the issues involved and without a theory of scientific change to back it up, it shows nothing about the relevance of such an appearance of paraphrase to the question of whether operant theory can succeed. action theory in Koertge's sense.

Rather than seeing the transition from animal behaviour to human action as demanding theoretical development of operant theory as the GCP allows, Chomsky adopts a naive falsificationist view--which, in addition, is employed with a misconceived interpretation of the theory in hand. Chomsky supposes he can refute operant theory

by showing that in the transition to human action there is a heavy reliance on internal sources of stimulation so that the original "objective" terms lose whatever objectivity they ever had. The problem, in Chomsky's words, is as follows:

The notions 'stimulus', 'response', 'reinforcement' are relatively well defined with respect to the bar-pressing experiments and others similarly restricted. Before we can extend them to real-life behaviour, however, certain difficulties must be faced. We must decide, first of all, whether any physical event to which the organism is capable of reacting is to be called a stimulus on a given occasion, or only one to which the organism in fact reacts; and correspondingly, we must decide whether any part of behaviour is to be called a response, or only one connected with stimuli in lawful ways.³⁸

In order to expose the error contained in this set of claims it might be useful to construct an analogous puzzle for classical mechanics. Imagine a physical object such as a block of wood at rest on a level frictionless plane. Is any physical event to which the object is capable of reacting (by moving) to be called a force on a given occasion, or only one to which the object in fact reacts? Clearly the former alternative is to be chosen. Since the plane is frictionless and no forces exist which oppose its movement in any direction on the surface, then any event, resulting in the object's movement is a force. Note that this fact does not preclude the existence of a

lawful relation between force, mass and acceleration. Nor is there any commitment to the statement that the same force will have the same effect under other conditions, i.e. on a non-frictionless plane!

But if this analogy is persuasive, and if the fact that any event that an organism is able to react to is a stimulus does not imply the absence of lawful relations, then Chomsky's conclusion simply does not follow:

Questions of this sort pose something of a dilemma for the experimental psychologist. If he accepts the broad definitions, characterizing any physical event impinging on the organism as a stimulus and any part of the organism's behaviour as a response, he must conclude that behaviour has not been demonstrated to be lawful... If we accept the narrower definitions, then behaviour is lawful by definition.³⁹

It does not seem all that strenuous to see that Chomsky is misguided here; but what leads him to this chain of reasoning is not so easy to extract. From this and other passages one might be able to infer that Chomsky does not see that only certain classes of stimulating events and only certain classes of physical movements need be said to be lawfully related. The problem is which classes and how. One begins to get the impression that Chomsky is referring to particular times and specific act-tokens. (He uses the phrase "on a given occasion" above.) He must be imagining that operant theory is committed to there being one set

response for every conceivable stimulus. As a matter of fact, the theory is not committed to there being any specific relation between specific stimuli and specific operant-tokens. Rather the theory says that whatever particular stimuli, responses, and reinforcements get associated there will always be an interaction between these three categories that takes a specific form. It is thus the form of causal generation that is specific, not the instance-types of the above three categories (let alone the instance-token). That is, the "event": "Thank-you" can be an instance-type of either of the three categories. It can be a response, a stimulus, and a reinforcer on "given occasions".

Taking a particular such occasion, if "Thank-you" is an instance of the category response, the theory is not suggesting there are specific instances of the other two categories which must be causally involved at this time. Chomsky fails to comprehend the generality of the theory as well as its thoroughly relativistic character at the level of the particular. The lawful relation holds between the three categories and specific events can be instances of different categories on different occasions for the same agent and in one situation for different agents. Moreover a single event can be an instance of two categories for the same individual at the same time. For example your response "Thank-you", can function to reinforce my giving something to you and to

serve as a discriminative stimulus for my response "You're welcome". Thus, your response is both a reinforcer and a discriminative stimulus for me. A single event can also function as a response and a discriminative stimulus as in when learning the alphabet the letter "C" as spoken is both a response and a discriminative stimulus for "D".

Chomsky thus conceives Skinner to be focusing entirely on particular operant-token occurrences and claiming to be able to predict and control them without knowledge of internal acquired structure. Skinner contributes to this confusion, for no such knowledge is required when one is dealing with organisms without any prior learning history. But as soon as the latter comes into play, prediction and control become only in principle possible, contingent upon information pertaining to internal structure.

Much the same can be said of Newton's laws. There is no implication that knowledge of the law alone permits prediction in the absence of information about all relevant initial conditions.

I shall now endeavour to demonstrate the claim that the foregoing confusion lies at the heart of Chomsky's entire critique. Recall that, according to Chomsky, "If we accept the narrower definitions, then behaviour is lawful by definition..."⁴⁰ This is precisely what Skinner does according to Chomsky. Referring to "stimulus" and "response" he claims that Skinner in The Behaviour of

Organisms:

...commits himself to the narrow definitions for these terms. A part of the environment and a part of behaviour are called stimulus... and response, respectively, only if they are lawfully related.⁴¹

In support of this reconstruction, Chomsky refers us to an early passage in The Behaviour of Organisms, where we find Skinner saying of "stimulus" and "response" the following:

The environment enters into a description of behaviour when it can be shown that a given part of behaviour may be induced at will (or according to certain laws) by a modification in part of the forces affecting the organism. Such a part, or modification of a part, of the environment is traditionally called a stimulus and the correlated part of the behaviour a response. Neither term may be defined as to its essential properties without the other.⁴²

(As an aside pertaining to the painfully slow process of scientific change notice that even in 1938, after Skinner's discovery of schedules of intermittent reinforcement, he still does not recognize the necessity of introducing the three-term concept contingency of reinforcement. He is still using the older S-R terminology even though he recognizes the significance of the third term--reinforcement.)

Returning to the main theme of the present discussion, we must ask: Is Skinner making the claim attributed to him by Chomsky, namely, that the terms "stimulus" and "response"

depend for their definition on the existence of a lawful relation between stimuli and responses? Clearly not.

From the fact that one cannot understand or define the terms without making reference to the two together, it by no means follows that there is any lawful relation obtaining between the referents of the terms, let alone the particular lawful relation we experimentally discover. Again the same holds of the terms "force", "mass" and "acceleration" and of the law $F = MA$.

MacCorquodale completely passes over in silence this crucial issue and instead begins his reply to this section of Chomsky's review, with a discussion of the example Chomsky employs. His reply consists simply of a restatement of the mistaken version of behaviourism whereby all relevant stimulus properties are actually part of the external physical stimulus. Thus in order to clearly see the manner in which Chomsky misconceives operant theory consider the following:

A typical example of 'stimulus control' for Skinner would be the response to a piece of music with the utterance Mozart or to a painting with the response Dutch. These responses are asserted to be under the control of extremely subtle properties of the physical object or event. Suppose instead of saying Dutch we had said Clashes with the wallpaper, I thought you liked abstract work, Never saw it before, Tilted...or whatever else might come into our minds... Skinner could only say that each of these responses is under the control of some other stimulus property of the physical object.⁴³

Indeed, with respect to the last sentence, Skinner gives little hint of any other interpretation. He does not explicitly explain that all of the above responses could be under the control of the same observable stimulus property with the exception perhaps of "Tilted". There is no need for a different stimulus property for every conceivable response, for the astonishingly simple reason that the stimulus property is only one member of a set of necessary conditions. In mature speakers most members of such sets will be features of their internal structure.

For example, suppose I have a certain history of reinforcement such that I am very particular about wallpaper and other decorative niceties. On simply seeing the painting for the first time, its relation to the wallpaper may be sufficient to raise the probability of my saying "Clashes with the wallpaper" above the requisite threshold. The internal state resulting from my history of reinforcement combines with the physical stimulus to increase the likelihood of my response. Another person with an analogous but slightly different history may need additional stimulation from his host--say to the effect: "Tell me honestly, how do you think this painting suits this room?"

Although both Skinner and MacCorquodale are to blame for not clearing up this confusion, Skinner does state occasionally that internal structure represents history of

reinforcement. No one has yet attempted to fill in the picture except Bandura, but even he does not make it satisfactorily clear precisely how he is developing operant theory. It seems apparent, in any case, that this confusion is due to the attribution of 4. to operant theory with its lack of reference to history of reinforcement and all that this concept implies. It is thus no wonder that Chomsky saw fit to ridicule the theory.

Chomsky also attributes 5. to operant theory: that the sole aim of the theory is the prediction and control of operant token occurrences. But even if this were the sole aim of the theory, no more than in principle prediction could be sustained. Yet Chomsky thinks that if the theory is true it should allow of the prediction and control of operant-tokens. Thus he asserts that "We cannot predict verbal behaviour in terms of the stimuli in the speaker's environment, since we do not know what the current stimuli are until he responds."⁴⁴ The truth of the matter is rather that we cannot predict the speaker's behaviour because we do not know which stimuli are asserting discriminative control for what response. And we do not know this because we do not know the speaker's history of reinforcement as represented by his current structure.

Another of the many instances of Chomsky's failure to see the function of history of reinforcement is as follows:

Elsewhere it is asserted that a stimulus controls a response in the sense that presence of the stimulus increases the probability of the response. But it is obviously untrue that the probability a speaker will produce a full name is increased when its bearer faces the speaker.⁴⁵

Clearly the manner in which Chomsky views operant theory is such that there must be some one stimulus which is both necessary and sufficient for the occurrence of each response. Operant theory, as Chomsky conceives it, is committed to the view that a person's coming face to face with an acquaintance is sufficient stimulation to cause the person to say his acquaintance's name.

Now, as I understand operant theory, very little development, if any, of its commitment to the role of internal structure is needed in order to see that Skinner never intended the interpretation Chomsky proposes. Skinner has always recognized the role of other necessary conditions besides the presentation of a stimulus, such as reinforcement history in particular, even though he may never have spelled out the role of such other conditions to everyone's satisfaction. History of reinforcement results in the existence of so many internal necessary conditions that mature humans can become relatively autonomous vis-a-vis external stimulation. It is imperative to realize that these admissions are entirely in keeping with a reasonable manner of developing operant theory. The conceptual framework of operant theory can in

principle encompass internal processes. Suddenly occurring ideas or thoughts would be said to play a role that is quite analogous to the role played by external discriminative stimuli.

Unfortunately, in order to bring out the obvious absurdity of Chomsky's interpretation of the concept of an operant, it is necessary to reproduce the following lengthy passage:

The unit of verbal behaviour--the verbal operant--is defined as a class of responses of identifiable form functionally related to one or more controlling variables. No method is suggested for determining in a particular instance what are the controlling variables, how many such units have occurred, or where their boundaries are in the total response. Nor is any attempt made to specify how much or what kind of similarity in form or 'control' is required for two physical events to be considered instances of the same operant. In short, no answers are suggested for the most elementary questions that must be asked of anyone proposing a method for description of behaviour. Skinner is content with what he calls an 'extrapolation' of the concept of operant developed in the laboratory to the verbal field. In the typical Skinnerian experiment, the problem of identifying the unit of behaviour is not too crucial. It is defined, by fiat, as a recorded peck or bar-press,....The operant is thus defined with respect to a particular experimental procedure. This is perfectly reasonable, and has led to many interesting results. It is, however, completely meaningless to speak of extrapolating this concept of operant to ordinary verbal behaviour. Such 'extrapolation' leaves us with no way of justifying one or another decision about the units in the 'verbal repertoire'.⁴⁶

There is so much confusion in this entire passage that it is difficult to decide where to begin. One gets the impression that Chomsky is unable to conceive of a definition in abstract terms. He thinks, quite absurdly, that an operant is defined as a key peck or a bar-press, thus restricting the concept to an operational level. But in spite of Skinner's operationist tendencies, there is no reason why the term "operant" cannot be defined as abstractly as Goldman defines "act-type".

It appears as though Chomsky expects to find some fixed and absolute unit of verbal behaviour such as the word, the sentence, or the proposition. Yet, as we have seen, an infinitely large class of verbal responses of varying lengths can count as single operants so long as they have the same effect, or exemplify a common property. For example, the operant of reprimanding one's child may consist of any set of events which exemplifies this operant/act property. It can consist of an infinite variety of verbalizations, not to mention facial and other gestures. On a particular occasion we may not know whether a particular string of words counts as the operant of reprimanding one's child because we may not know the reinforcement histories of the individuals involved. That we cannot specify the operant on particular occasions in no way precludes a definition of the term "operant" that is not context dependent. This discussion should serve to

vitiate the major charges included in the above passage. Clearly, the difficulties surrounding the problem of act or operant individuation infect the writings of both Chomsky and Skinner.

A number of other outstanding errors will now be dismissed in as brief a manner as possible. For example:

It is not clear how the frequency of a response can be attributable to anything BUT the frequency of occurrence of its controlling variables if we accept Skinner's view that the behaviour occurring in a given situation is 'fully determined' by the relevant controlling variables.

But the former by no means follows from the latter and indeed the conclusion is quite false. Chomsky has apparently forgotten intermittent reinforcement. On a fixed ratio schedule as many as eighty responses could be required for a single reinforcement. Here the frequency of responses is fixed relative to the frequency of reinforcement. But on variable ratio schedules, only a certain average number of responses is required for a single reinforcement. Rather than an atomistic one stimulus-one response relation we have something rather more fluid occurring under reinforcement schedules. A schedule maintains a steady state so that the conception of long chains of atomistic S-R connections is quite misguided.

I have now commented on Chomsky's 'mistaken interpretation of the concepts of stimulus and operant. In order

to avoid repetition I shall forego discussing his remarks on the concept of reinforcement. Since they pertain to the alleged tautologousness of the "law of reinforcement" and as I have already spelled out my position on this issue, further comment is unnecessary.

My conclusion is thus that Chomsky has completely failed to impugn operant theory in any but the naivest of falsification senses. The extent to which operant terms are paraphrases of mentalistic terms and damaging to the foundations of the theory has not been established. I suspect that it will turn out that there is a good deal of truth to both approaches and that a fruitful integration of the two substantive theories can be anticipated.

I have now discussed the foundations of operant theory as it pertains to the behaviour of individuals. It is thus time to turn to a consideration of the possibility of a social operant theory. I shall attempt to ascertain the potential of operant theory as a foundation for the social sciences. But before beginning this task, a brief summary of the foregoing is in order.

There is good reason, I have argued, for accepting some aspects of the methodological foundations of operant theory and equally good reason for rejecting others. The temporary avoidance of hypothesizing internal mechanisms has been undeniably fruitful in setting up a rigorous science of behaviour. In spite of the very good grounds

for attempting to integrate operant theory with other approaches in psychology, a systematic comparison of man and other animals demands a tenacious restraint against premature generalization of the role of cognitive or other hypothetical processes and states. Such an approach is essential to the purpose of isolating the peculiarly human. On the other hand, such restraint is entirely in keeping with a more liberal and realist attitude to theory construction.

Most of my discussion has concerned theoretical and conceptual foundations. It is in principle possible, or so I have argued, to replace the concept of an act by that of an operant, the concept of a rule by that of a reinforcement contingency, the concepts of wants and beliefs by those of ψ states and ϕ states respectively, and finally, the concept of cognitive processes by that of covert operant behaviour. The latter claim carries with it no claim regarding the specific ontological form of such processes. It can be left open whether they are physical, chemical, electrical, mental, or whatever.

Broadening operant theory to include a place for internal structure, covert processes, and act/operant acquisition by exposure to contingencies of reinforcement that is other than direct, i.e. by model observation, does not constitute dogmatism unless one subscribes to a narrow Popperian falsificationism. As I envisage the purported

theory change, the resulting "operant" theory will be such as to yield current animal operant theory as one special case and a qualified version of Goldman's theory for humans as another special case. Nothing hangs on either the retention or the abandonment of the term "operant theory" as a name for the envisaged successor theory. It is somewhat of a verbal issue whether we are to speak of theory change as involving the abandonment of old theories as opposed to their revision. That is, the abandonment of the name "operant theory" is not tantamount to a rejection of the theory's central insights. As the GCP indicates, old theories are revised beyond recognition, rather than explicitly abandoned wholesale.

The diametric opposition we find throughout philosophy is absent from the present essay. The resulting operant theory is neither mechanistic nor teleological in any interesting or traditional sense. It is neither behaviouristic nor non-behaviouristic. Rather it captures the insights of many such traditions and therein lies the source of its primary explanatory power.

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- ⁹ Heidbreder, op. cit., pp. 250-251.
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VI. OPERANT FOUNDATIONS OF THE SOCIAL SCIENCES

Increasingly in the last decade or so, sociologists have begun to look to operant theory for a foundation of a theory of social processes. If they are on the right track in this respect, it will be patently obvious that operant theory is of fundamental importance for a proper understanding of the behaviour of all organisms. Although recent attempts to so found the social sciences may be somewhat premature, they are certainly a welcome sign to anyone interested in the "unity of science" point of view. Before we can properly assess the potential of operant theory as a foundation of social science, however, the structure of operant theory must itself be adequately founded, clarified, and formalized. Nonetheless, since my primary concern is to motivate philosophical interest in the complex formal and foundational problems involved, a brief and informal exploration of various suggestions and problem areas is in order. Accordingly, with the aim of provoking such interest, I shall attempt a critical sketch of the work that has been done and indicate possible areas of further research. I shall also consider the relation of operant theory to rational choice theory in order to obtain some hint as to which of the two theories is more basic.

The leader of the new "behavioural sociology" to date has undoubtedly been George Homans. Since the publication of the first edition of his Social Behaviour: Its Elementary Forms in 1961, there have been few attempts to follow his lead. However, a recent and very important collection of papers should stimulate wider interest. In Behavioural Sociology, 1969, edited by R. L. Burgess and D. Bushell, the most significant contribution is a paper by Richard M. Emerson entitled "Operant Psychology and Exchange Theory." The importance of Emerson's effort consists in the high degree of rigor with which he sets out to found exchange theory upon operant theory. Although the above is largely an informal survey of his approach, Emerson has a much more rigorous and extensive treatment published elsewhere.¹

Naturally the adequacy of operant theory to serve as a foundation for the social sciences does not immediately follow from its ability to found exchange theory. I shall not argue this aspect of the question but instead simply mention that exchange theory is not of isolated interest in the social sciences. Other sociologists besides Emerson such as Alfred Kuhn and Peter Blau regard exchange theory as fundamental, as does Homans in both his original and his recently revised edition of Social Behaviour.

Over and above the difficulties of working out the structural relation between operant theory and exchange theory, there are three main issues involved in the Homans-

Emerson programme. Discussions of exchange theory, in Emerson's words:

...seem to center around (a) the role of rationality in social behaviour, (b) the possibly tautological character of what are meant to be basic propositions, and (c) strategic questions concerning 'psychological reductionism.'²

The major questions of the three listed by Emerson are (a) and (c). The issue, stated in (b) is, I believe, something of a pseudo-problem. Surrounding any new general theory there is always an air of suspicion of tautologousness.

When the theory becomes generally accepted or supersede the suspicion dissolves. Hence I shall concentrate upon the other two issues beginning with (c)--the question of reduction. But first a brief comment on (b) is in order.

In attempting to obviate the charge of tautologousness, Emerson refers to statements of the law of effect in the writings of Homans and Skinner and then claims that,

In the above statements, both Homans and Skinner are assuming that we know from independent evidence which specific stimuli are rewarding or reinforcing to which men.³

But this will hardly do. If "All bachelors are unmarried males," is a tautology, it does not become an empirical proposition by virtue of our knowing which particular men in the world are bachelors and which are not.

On the other hand, there is no a priori demonstration of the non-tautologousness of such putative general laws. Their empirical character--like Newton's laws--is simply revealed in the process of ongoing science whereby scientific theories replace one another. Although this process is poorly understood, the simple fact that the charge of tautologousness is levelled over and over again throughout the history of science should induce us to take the charge with a grain of salt. Surely it follows that the burden of proof is upon those who would impute tautologousness to a general law. Both Emerson and Skinner go too far in their efforts to avoid the charge of tautologousness. In so doing they deprive operant theory of its explanatory power. In Emerson's words, (and Skinner says exactly the same thing), "A does not perform x because he finds y 'rewarding'"⁴ But surely it makes sense to say that a particular acceleration occurs because of a specific force in spite of the fact that force and acceleration are interdefined. And it is not that we know from independent information which events are forces; this knowledge derives only from our measurement of the effects of force on mass.

I shall now attempt to determine what it could possibly mean to reduce exchange theory to operant theory. Although my comments on Emerson's efforts will be entirely critical, I must emphasize that I regard his work as the

most fundamental start in the direction of founding social science on operant theory to date. My central aim will be to emphasize the great need for deeper foundational research on operant theory. That sociologists are expending considerable energy endeavouring to found their theories on operant theory is ample testimony alone. Further inducement should derive from a brief survey of the area.

Anyone familiar with the low level of formalization in operant research can easily see the extent to which Emerson has to start from scratch. Thus, in order to avoid ambiguities, Emerson tells us, it is necessary to introduce his own formal machinery such that he must "begin by stating basic operant concepts with unusual precision."⁵ I shall not discuss in any detail the adequacy of his particular semi-formalization, for it is precisely this very large task that the present discussion is intended to motivate. But I shall state, briefly, a few reservations I have about his formalization. Firstly, just as I believe that philosophers intimately familiar with quantum mechanics are in the best position to work out the foundations of that theory, so I am convinced that philosophers should be in a position to contribute immensely to foundational research on operant theory. The underlying conviction that is operative here is that it is absurd for philosophers to restrict their attention to our common-sense concept of action, as absurd as it would be for

philosophers to restrict their interest in physical concepts such as space and time to our commonsense intuitions of them.

Thus an adequate formal foundational research programme will not likely come from sociologists but from philosophers and operant psychologists working hand in hand. This points to a second source of my reservations regarding Emerson's attempt which is that there has, very recently, been a modicum of formal foundational publication coming from within the operant community. This work has been too recent to have had an effect on Emerson. R. J. Herrnstein has led the way in this recent formal approach and his recent paper "Formal Properties of the Matching Law"⁶, is representative. Whereas Emerson has attempted to formalize the law of effect, Herrnstein has been working out a principle to replace the law of effect, but one which captures the latter's insights. Herrnstein makes substantial claims on behalf of his principle:

It has been repeatedly shown that animals (including human beings) distribute their behaviour across alternatives according to the matching principle.⁷

After stating the principle, he makes the rather bold claim that:

...matching appears to be a general formal framework to replace the traditionally informal (or non-formal) vocabulary for the law of effect.⁸

Herrnstein's efforts to reconstruct operant theory at a formal level are not only at the very foundations of the theory, but also on the very frontier of such research. Since it is clear that this work barely scratches the surface of this area, no statement of the matching principle will be provided here. Nor will I attempt to assess Emerson's formal structure in light of Herrnstein's

principle. I merely call attention to the latter to express how urgently such research is needed. In what follows I shall rather be concerned with the much more modest task, as earlier stated, to attempt to ascertain what it could mean--conceptually--to found exchange theoretical concepts upon operant concepts. Obviously my discussion will be no more than suggestive, but hopefully of heuristic value, since the currently developing formalization of operant theory will result in not only a more precise statement of operant concepts, but also very likely in somewhat altered conceptualizations of the intuitively understood operant terms I have been examining.

In order to answer the question of how social science in general and exchange theory in particular might "reduce" to operant theory it is clear, as it has been throughout the foregoing, that an absolutely necessary prerequisite is an adequate theory of reduction or scientific change. It should also be clear that, in the present case, we do not have precisely the same sort of reduction or theory

shift as the one I have attempted to establish between operant and action theory. In the latter case, the two theories compared were appropriately viewed as competitors for the same domain. That putative theory change is analogous to the one between the phlogiston and oxygenation theories of combustion where, although the former could be regarded as an approximation to the latter, it was correct to view the phlogiston theory as false. Now in the alleged reduction of exchange theory to operant theory the above analogy is inappropriate. The relation between the two theories appears, at first glance at least, to be of the macro-micro sort. Ostensibly, operant theory pertains to the behaviour of individuals whereas exchange theory involves social interaction. But there does not appear to be any ground for not regarding groups as individuals as well, in which case the alleged "micro" character of operant theory disappears. Moreover, since I have argued that operant theory is most like the theory of evolution, and since the latter pertains not to the behaviour of individuals but rather to the developmental change of species (types), it becomes even more difficult to envisage operant theory as a "micro" theory. Just as the theory of evolution pertains primarily to the change of molar types, so operant theory is a putative account of the development of the molar act-types of mankind. One can only regard operant

theory as a micro theory to the extent that one tends toward methodological individualism and the interpretation of operant theory whereby the behaviour of individuals at particular times is at least the primary concern. There are thus several unsolved problems to which I have barely alluded involving the nature of reduction and the correct interpretation of operant theory which stand in the way of a satisfactory assessment of Emerson's claim to be constructing a theory of social exchange upon operant theory rather than reducing the former to the latter. Nonetheless, I shall comment on Emerson's efforts and attempt to suggest a plausible physical science analogy in order to get some grasp of his programme that may be at least of heuristic value. No final answer to the question of whether exchange theory "reduces" to operant theory, let alone in what sense, will be attempted here.

Accordingly, in order to stimulate further interest in the foregoing questions, I shall attempt to make a few intuitively plausible suggestions about what might be involved in the following contention of Emerson's:

To say that sociological exchange theory can be "founded upon" operant psychology implies two things. First, exchange theory can incorporate and use propositions from operant psychology. And second, exchange theory is different from or adds something to operant psychology.⁹

Now, as I have already mentioned, it must be kept in mind that Emerson views operant theory as logically prior to exchange theory in the sense that the former is viewed as involving primarily the behaviour of individuals whereas the latter is centrally concerned with social interaction. Exchange theory in turn is logically prior to social structural theories and concepts. But, in assessing this programme we must keep the vaguely suggested possibility in mind that exchange is no more logically prior to social structure than current chicken eggs are to current chickens. In other words, operant theory may be prior to exchange theory, not in the sense of being fundamentally an account of individual behaviour, but rather in the sense of providing a step-wise evolutionary account of the development of both current forms of social exchange and current forms of social structure. The result would be a more organismic-holistic view of social structure and a radical rejection, rather than a minor modification, of the traditional empiricist individualist view with its basic ego-centric standpoint. An examination of this topic is far beyond the scope of the present inquiry since my primary concern has been with the relation between operant and action theory.

I simply mention it to stress that while I shall stay within the general framework Emerson presupposes, the

possibility of fairly radical alternative social science. implications of operant theory must at least be kept in mind. There is a potential elementary confusion that must be avoided. One might object that if we are interested in social structure it is irrelevant to refer to the evolution or development of that structure; two separate topics are involved. This is true of course, but my question pertains to which of these topics is operant theory of most foundational significance.

What, then, does Emerson mean by the contention that exchange theory is "different from" and "adds to" operant theory? Since I am going to isolate a few points for discussion rather than reconstruct the whole programme, some familiarity with the latter is presupposed in what follows. A theory of reduction is necessary in order to answer the above question, but I have already suggested that the examples I have employed in my use of the GCP are very likely not-analogous to the present use. I have also expressed qualms about the possibility of construing the putative "reduction" along macro/micro lines. But for the sake of argument I shall suppose that one possible analogous case of reduction might be that of thermodynamics to statistical mechanics, with operant theory regarded as playing a role analogous to the latter and exchange theory a role similar to that of the former. It will emerge that the operant/exchange "reduction" appears

to have some properties in common with such a case as the above. But it may be illuminating to explore two other possible analogies as well. One such would involve a reductive relation obtaining between two molar theories such as Newtonian dynamics and fluid dynamics. Another would be between the contribution of Copernicus to the Newtonian world view and the latter itself. In the latter case we would view operant theory as consisting of a small set of useful insights and concepts around which we would build a social interactional theory analogous to the supplementing of the insight of Copernicus with the planetary theory of Kepler, the mechanics and dynamics of Galileo and especially of Newton. Operant theory would then be foundational only in this loose constructive sense which Emerson appears to have in mind.

With either of the first two foundational analogies in mind we would expect all of the concepts of the reduced theory--in this case exchange theory--to be definable in some sense in operant terms.. No such reduction would be even remotely forthcoming on the Copernican/Newtonian analogy.

In order to explore the relation between operant and exchange theoretic concepts it might be useful to have an example at hand. If a human interactional example is employed there will be too many dimensions to enable us to ascertain clearly the role of exchange concepts. Accord-

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ingly, I shall employ an example involving cooperational interaction or exchange between two pigeons. If operant theory cannot even conceptualize this example fully, then it will be clear that its likelihood of accounting for human exchange is even more remote. I shall argue that exchange theoretic concepts are useful even at the level of the explanation of animal interaction. It is interesting to note that Emerson thinks operant research has been restricted to individuals; but if he had considered animal interaction he might have been in a position to present a stronger and clearer case. The example I shall discuss is a demonstration experiment Skinner designed years ago involving two pigeons that could not obtain food reinforcement unless they cooperated. The pigeons were

...in adjacent compartments...separated by a pane of glass. Three red buttons were arranged in a vertical row on each side of the glass...In the final performance, both pigeons were reinforced with food when they pecked a corresponding pair of buttons so nearly simultaneously that the brief closures of the circuits (each lasting perhaps a tenth of a second) overlapped. At any given time, however, only one pair of buttons was operative, and the effective pair was scheduled in a roughly random way.

It was necessary for the pigeons to cooperate in two tasks: (1) discovering the effective pair and (2) pecking both buttons at the same time...One pigeon (the "leader") explored--that is, it struck the three buttons in some order...The other pigeon (the "follower") struck the button opposite that being struck by the leader.¹⁰

It should be abundantly apparent that this example does indeed exemplify an exchange relation on Emerson's view, since the concept of exchange is introduced in order to

facilitate more macroscopic analysis of behaviour in contexts far removed from operant experiments...in which transactions are reciprocally reinforcing events which can be "initiated" from either end of the relation.¹¹

Each pigeon in the example is reciprocally reinforced by the exchange and each can initiate the transaction by virtue of assuming what Skinner calls the leader role. What can operant theory tell us about this interactional behaviour? By citing the respective reinforcement histories of the two pigeons, it can account at least partly for the particular behaviour occurring. It can describe the specific contingencies of reinforcement operative in the situation described by Skinner. But can it explain why these particular contingencies are controlling the behaviour of the pigeons at this time?

One concept central to Emerson's programme that would appear to be required for explanatory purposes in the above example is the concept of dependence. The intuitive idea is that part of the explanation of the exchange relation is that the two pigeons depend on each other for reinforcement. There are no other alternative sources of food.

The reason other behaviour under the control of other contingencies does not occur is because, in order for the pigeons to obtain what they maximally value at this time they must depend on each other. Therefore, the particular contingencies which in fact govern the requisite exchange behaviour are in fact controlling. I shall return to the role of maximization later; but first consider the concept of dependence further. According to Emerson, "dependence varies directly with the value of the domain(s) involved, and inversely with the number of alternatives..."¹² Accordingly, in the present case, the requisite contingencies do in fact operate because the value of the reinforcement is high and the number of alternatives is non-existent, which is to say that the pigeons are under the control of certain contingencies because they depend on them.

Our question is therefore whether there is any relation between some specific operant concept and the concept of dependence. The answer is clearly in the negative; operant researchers simply presuppose dependence without ever explicitly mentioning it. Part of the reason for this must surely be their atheoretical bias and resulting satisfaction with description. But it is obvious that dependence is an important and unique concept in its own right. Instead of employing it in an explanatory account of behaviour, operant experimenters simply

induce it operationally for research purposes. It is induced in the following way: The value of some arbitrarily chosen reinforcer is increased by depriving the animal of it and the number of alternatives is decreased by simply insuring that there are none at all. Accordingly, the concept of dependence has a useful explanatory role to play even in single individual operant theory and research; it is neither a "macro" concept defined in terms of a "micro" counterpart nor even defined in operant terms at all. Hence the Copernican/New tonian analogy begins to emerge as the most suggestive of those mentioned above. Operant experimenters have explored the interrelations among discriminative stimuli, reinforcement and operant-type generation and between reinforcement schedules of various sorts and particular operant-type steady patterns, but, like the Copernican insight, it is only a beginning. There are no conceptual bars, as I have argued, preventing the succeeding of action theory by operant theory, but this still is just a beginning toward a complete account of behaviour--especially human social behaviour.

Once other concepts Emerson introduces are discussed it will be immediately obvious that their status is precisely the same as that of dependence. I shall discuss two such concepts, balance and power, simply to further demonstrate that something like the Copernican analogy is indeed apt. Consider balance first and recall the pigeon

experiment. An exchange relation is said to be balanced if the dependence of one party on a second is equal to that of the second upon the first. As Emerson puts it, "Balance in the relation is...a function of four variables: value and alternatives for A and for B."¹³ In the pigeon experiment both pigeons are equally lacking in alternatives. So long as their level of food deprivation is approximately the same, the value of the reinforcer will be the same for both and the exchange relation will be balanced. It should also be clear that this concept is of explanatory importance even in accounting for individual behaviour in as much as the value or reinforcing power of objects and the availability of alternative sources plays a causal role.

Because the concept of power is redundant in Emerson's scheme, it too is a concept that has explanatory relevance without being definable by, or reducible to, operant concepts. In the above experiment one pigeon has power over another to the extent that the second is more dependent on the first. The power advantage of one pigeon is equal to the degree of imbalance in the exchange relation in its favor.

It is easy to see how these concepts fit in with operant theory and serve to extend it in illuminating ways. Consider the operant theorist's concept of control. We can now define this concept so that an organism or even the environment (the contingencies), can be said to have a

degree of control over an individual that is equal to its degree of power. Actually control is the actualization of power, though operant theorists have never bothered to spell out such conceptual linkages.

There is a further way in which operant and exchange theory complement each other. It is unfortunate that Emerson does not examine the concept of a contingency of reinforcement; for if I am right in thinking that rules are reinforcement contingencies, then if exchange relations are governed by rules, they are governed by reinforcement contingencies. Moreover, Emerson does consider norm formation and concludes that it results from a certain type of interaction: coalition formation. But again, if norms, which are rules, are contingencies of reinforcement, we can regard coalition formation as the generation of social contingencies of reinforcement. Accordingly, the latter concept is of great foundational significance with respect to theories of social processes as well as with regard to the behaviour of individuals.

It thus emerges that something like the Copernican/Newtonian analogy is most suggestive in an attempt to discern the relation between operant and exchange theory. The Copernican idea needed supplementing by dynamical concepts (inertia, for example) and a dynamical theory in order to provide a fully adequate picture of molar bodies in space and the solar system in particular.

Similarly, we can regard the addition of exchange theoretic concepts to operant theory as an attempted completion of the picture of behaving, interacting organisms. Exchange theory cannot be reduced to operant theory in spite of Emerson's use of the micro/macro contrast. Whether the latter metaphor is applicable will await deeper study of the putative evolutionary character of operant theory.

For present purposes I shall conclude my sketchy comments on the foundations of social science with some suggestions of still further possible ways of filling out the operant-exchange framework. I have in mind the remaining major issue Emerson regards as essentially involved in any attempt to "found" social theories on operant theory: What is the role of rationality in social science? Since Emerson does not discuss this extremely important matter I shall simply present my own speculative suggestions.

One concept central to some theories of rational choice is, of course, maximization of expected utility. I shall argue that some such concept is necessary to further develop operant theory in precisely the same fashion as exchange theoretic concepts were found necessary. Operant theory alone can account for particular act-types and token occurrences in terms of particular histories of reinforcement, schedules of reinforcement, and prevailing contingencies of reinforcement. But one thing it cannot

explain is why contingencies of reinforcement in fact operate as Skinner has found them to operate. Even in the most restricted operant experimental situations, subjects have many alternatives. Why should the maximally reinforcing alternative be chosen. Obviously operant-types and patterns of such can only be shaped by differential reinforcement and maintained by schedules of reinforcement respectively under some such assumption as that organisms seek to maximize their welfare in some sense. It thus appears that some such theory of rational choice as those that rely on the notion of maximization of expected utility explains how it is that operant conditioning is able to work. Skinner can no more explain why any reinforcers should work and should order themselves as they do than Newton could explain why gravity worked. But it is a fact in need of explanation that the highest probability of response is always associated with reinforcers with the greatest reinforcing power. It seems, again, that operant researchers have failed to see and develop this aspect of the foundations of their theory due to their extreme atheoretical bias.

Accordingly, operant theory can explain how, for some, agent, x has come to have more reinforcing power than x^1 in terms of x 's having been associated in a certain manner with other reinforcers. But it cannot explain why any such x and x^1 should always order themselves thusly and why the

most likely behaviour should be associated with x rather than x^1 .

But now, puzzling questions arise. Is the theory of rational choice as it pertains to human action also the foundation of operant theory as it pertains to all animals? Or is there a more general theory which yields versions of the theory of rational choice that are somewhat distinct for animal and human operant behaviour? This puzzle becomes even more mystifying when the existence of a high degree of similarity between operant theory and the theory of evolution by natural selection is noticed. Reinforcement selects and gives rise to new act-types much as natural selection does for the evolution of species. But must we now say that rational choice theory explains evolution too? It is noteworthy that we do not in fact have an answer to the questions: Why does the fittest survive? Why do species develop in such a way so as to always "maximize their utilities"? Just as only those variations among act-types which are maximally reinforcing survive, so only those species which develop so as to preserve or maximize their advantage survive.

If these questions show anything at all it is that we know next to nothing about the interrelations between evolutionary theory, operant theory, and rational choice theory. It does appear that some such notion as maxi-

zation of expected utility underlies and explains natural selection and operant conditioning, but at this point, such an intuition is of no more than heuristic value. Obviously, this is an area that is extremely large and as such constitutes an enormous research programme.

I shall now simply draw together the various strands I have been pursuing and draw some merely suggestive conclusions.

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² Ibid., p. 39.

³ Ibid.; p. 40.

⁴ Ibid., p. 45.

⁵ Ibid., p. 44.

⁶ Herrnstein, R. J., "Formal Properties of the Matching Law", Journal of the Experimental Analysis of Behaviour, 1974, 21, pp. 159-164.

⁷ Ibid., p. 159.

⁸ Ibid., p. 159.

⁹ Emerson, R., "Operant Psychology and Exchange Theory", in R. Burgess and D. Bushell, Columbia U.P., 1969, pp. 379-380.

¹⁰ Skinner, B.F., in The Experimental Analysis of Social Behaviour, R. Ulrich and P. Mountjoy (eds.), Appleton-Century-Crofts, 1972, p. 15.

¹¹ Emerson, op. cit., 1972, p. 45.

¹² Ibid., p. 59.

¹³ Ibid., p. 62.

SUMMARY AND CONCLUDING COMMENTS

If any contention is very secure as a result of the foregoing arguments it is surely that an understanding of the nature of scientific change is essential to an examination of the nature and status of such theories as operant theory. Such age old disputes as that involving mechanism and teleology can be seen in a whole new light with an adequate view of scientific change in hand. One of my further major claims--which I also feel has been shown reasonable--is that a proper understanding of operant theory shows that it is neither mechanistic nor teleological in any interesting sense.

With the aim of generating deeper interest, as well as further conceptual and formal investigation of the structure and foundations of a valuable core of operant theory, I have provided some detail of one conceivable way in which a revised version of operant theory can, in principle, replace theories of human action such as that of Alvin Goldman. It seems intuitively clear that the relation between action and operant theory is analogous to the relation between the phlogiston and oxygenation theories as understood by the GCP, so that much of action theory survives the conceptual transition in a slightly qualified form.

Once positivistic doctrines are repudiated, the way is open for the inclusion of internal processes, states, and events, and more importantly, to an operant description and explanation of them. Once the older, narrower, atomistic, S-R version of behaviourism is fully exorcised the door is at least open for the development of a fully evolutionary interpretation of operant theory in which the concept contingency of reinforcement is fundamental. The rejection of the atomistic S-R overtones to current views of operant theory frees the latter to recognize, more explicitly, other than direct unconscious ways in which exposure to contingencies of reinforcement can have causal efficacy.

And, lastly, the rejection of narrow reductionism allows us to view operant theory as a set of insights much like the Copernican theory upon which we can build so as to link operant theory fruitfully with the theory of evolution, on one hand, and with a theory of social processes on the other. This task and the one pertaining to the role of rational choice theory is beyond the scope of the present inquiry. I shall be satisfied if I have shown only that such a programme is well worth exploring and that operant theory is far from going the way of logical positivism on the current intellectual scene.

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